

THE SYNTAX OF PHONOLOGY

by

JOEL ROTENBERG

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Signature of Author.....  
Department of Linguistics  
and Philosophy, May 26, 1978

Certified by.....  
Thesis Supervisor

Accepted by.....  
Chairman, Departmental Committee



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## ABSTRACT

This study is concerned with the proper modes of syntactic influence in phonology. It is proposed and proven that this influence is direct, not mediated by string boundaries. A contextually applicable constituency predicate is developed to handle certain phonological rules applicable at gross phrasal levels which clearly have syntactic conditioning. Instanced are French liaison, Hebrew accentuation, and Italian raddoppiamento. A special case of the required notion of constituency is standard level theory. Thus a general account of domains of application results.

There are certain structural configurations which universally, it is suggested, block rules of a certain independently specifiable type. Blockage of rules across trace, parenthesis, and edge of sentence is blamed on a single structural defect, so that rule failure at any one of the three will entail failure at the other two. Extensive case studies: English phrasal phonology and the Irish initial mutations. Finally, a single rule of English, final dental palatalization, is inspected in detail as an example of a rule not subject to any lexico-syntactic conditioning, and predictably so: The rule is conditioned by prosodic structure instead.

A recurrent theme is the inadequacy and inappropriateness of terminal boundary symbols as a segmental means of reflecting non-segmental information.

Thesis Supervisor: Noam Chomsky

Title: Professor of Linguistics

For the things which are seen are temporal;  
But the things which are not seen are eternal.

2 Cor. iv 18.

The omission or the addition of one letter  
might mean the destruction of the whole world.

Talmud.

The world is nothing but a big gimmick.

Hendrix.

Téora seithir góa: Bés, dóig, toimte!

To Albert Hofmann



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## INTRODUCTION

This thesis began as a collection of smallish, more or less discrete studies in the general area suggested by the rather vague title--syntax-phonology interactions. As my understanding of the several topics grew, or seemed to grow, so did their mutual dependence and relevance, until the conclusion I was reaching about each one became tied up in a really organic way with the conclusions I found myself reaching about the rest.

I think it would be wrong to say that the four parts of this thesis, taken together, go to show any one coherent thing. However, it develops that there is an overriding theme. In each of the individual problems I examined, a fairly natural account in terms of word boundaries in the manner of Chomsky and Halle (1968) (henceforth SPE) immediately offered itself. Upon closer inspection, it turned out to be wrong. In fundamental, though unobvious respects, it looked like boundaries were not doing what they were supposed to be doing. I began to wonder what boundaries did right. It now seems to me that they and all other boundary symbols are dispensible, and in fact rather misleading notational devices at best.

Let us consider briefly what a word boundary is. A word boundary, in SPE theory, is a way of encoding terminally certain selected aspects of pre-terminal syntactic structure. We should know, first, exactly what the boundary conventions do.

SPE is quite clear, to a point:

BC I: The boundary # is automatically inserted at the beginning and end of every string dominated by a major category, i.e. by one of the lexical categories "noun," "verb," "adjective," or by a category such as "sentence," "noun phrase," "verb phrase," which dominates a lexical category.

(SPE, p. 366.)

Operating in conjunction with BC I is a convention of #-telescoping, unfortunately never openly enunciated in SPE. As interpolated by Selkirk (1972), it goes

BC II: In a sequence 
$$\begin{array}{ccccccc} W & \# & \boxed{1} & \# & \boxed{1} & Z & \text{or} & W & \boxed{1} & \# & \boxed{1} & \# & Z, \\ & & X & & Y & & & & Y & & X & & \end{array}$$

where  $Y \neq \bar{S}$ , delete the "inner" word boundary.

(Selkirk, p. 12.)

An example.

(1) John has a large trout in his pocket.

Let's suppose that this sentence comes with the bracketing in (2).

(2) 
$$\begin{array}{cccccccccccccccc} \boxed{1} & \boxed{1} & \boxed{1} & \text{John} & \boxed{1} & \boxed{1} & \boxed{1} & \text{has} & \boxed{1} & \boxed{1} & \boxed{1} & \boxed{1} & \text{a} & \boxed{1} & \boxed{1} \\ \bar{S} & \bar{S} & \bar{S} & \bar{N} & \bar{N} & \bar{V} & \bar{V} & \bar{V} & \bar{V} & \bar{V} & \bar{N} & \text{DET} & \text{DET} & \bar{N} \end{array}$$

$$\begin{array}{cccccccccccccccc} \boxed{1} & \boxed{1} & \boxed{1} & \text{large} & \boxed{1} & \boxed{1} & \boxed{1} & \text{trout} & \boxed{1} & \boxed{1} & \boxed{1} & \boxed{1} & \text{in} & \boxed{1} \\ \bar{A} & \bar{A} & \bar{A} & \bar{A} & \bar{A} & \bar{A} & \bar{A} & \bar{N} & \bar{N} & \bar{N} & \bar{N} & \text{PP} & \text{P} & \bar{P} \end{array}$$

$$\begin{array}{cccccccccccccccc} \boxed{1} & \boxed{1} & \boxed{1} & \text{his} & \boxed{1} & \boxed{1} & \boxed{1} & \text{pocket} & \boxed{1} & \boxed{1} & \boxed{1} & \boxed{1} & \boxed{1} & \boxed{1} \\ \bar{N} & \text{DET} & \text{PRO} & \text{PRO} & \text{DET} & \bar{N} & \bar{N} & \bar{N} & \bar{N} & \text{PP} & \bar{V} & \bar{S} & \bar{S} \end{array}$$

BC I provides for boundaries as in (2)'.

(2) '  $\begin{array}{ccccccc} \underline{[} \# \underline{[} \# \underline{[} \text{John} \# \underline{]} \underline{[} \# \underline{[} \# \underline{[} \text{has} \# \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{a} \underline{]} \underline{[} \# \\ \text{S} \text{ S } \underline{\text{N}} & \underline{\text{N}} \underline{\text{V}} \underline{\text{V}} \underline{\text{V}} & \underline{\text{V}} \underline{\text{V}} \underline{\text{N}} \text{ DET} & \text{DET } \underline{\text{N}} \end{array}$

$\begin{array}{ccccccc} \underline{[} \# \underline{[} \# \underline{[} \text{large} \# \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{trout} \# \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{in} \underline{]} \underline{[} \# \\ \text{A} \text{ A } \text{A} & \text{A} \text{ A } \underline{\text{A}} \underline{\text{N}} & \underline{\text{N}} \underline{\text{N}} \underline{\text{N}} \text{ PP } \text{P} & \text{P} \end{array}$

$\begin{array}{ccccccc} \underline{[} \# \underline{[} \underline{[} \text{his} \underline{]} \underline{]} \underline{[} \# \underline{[} \# \underline{[} \text{pocket} \# \underline{]} \underline{]} \underline{]} \# \underline{[} \# \underline{[} \underline{]} \underline{]} \# \underline{[} \# \\ \underline{\text{N}} \text{ DET } \text{PRO} & \text{PRO } \text{DET } \underline{\text{N}} \underline{\text{N}} & \underline{\text{N}} \underline{\text{N}} \underline{\text{N}} \text{ PP } \underline{\text{V}} \text{S } \underline{\text{S}} \end{array}$

BC II disposes of most of these, resulting in (2)".

(2)"  $\begin{array}{ccccccc} \underline{[} \# \underline{[} \# \underline{[} \text{John} \# \underline{]} \underline{[} \# \underline{[} \underline{[} \text{has} \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{a} \underline{]} \underline{[} \# \\ \text{S} \text{ S } \underline{\text{N}} & \underline{\text{N}} \underline{\text{V}} \underline{\text{V}} \underline{\text{V}} & \underline{\text{V}} \underline{\text{V}} \underline{\text{N}} \text{ DET} & \text{DET } \underline{\text{N}} \end{array}$

$\begin{array}{ccccccc} \underline{[} \underline{[} \underline{[} \text{large} \underline{]} \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{trout} \underline{]} \underline{]} \underline{]} \# \underline{[} \# \underline{[} \text{in} \underline{]} \underline{[} \# \\ \text{A} \text{ A } \text{A} & \text{A} \text{ A } \underline{\text{A}} \underline{\text{N}} & \underline{\text{N}} \underline{\text{N}} \underline{\text{N}} \text{ PP } \text{P} & \text{P} \end{array}$

$\begin{array}{ccccccc} \underline{[} \# \underline{[} \underline{[} \text{his} \underline{]} \underline{]} \underline{[} \# \underline{[} \text{pocket} \underline{]} \underline{]} \underline{]} \underline{]} \underline{]} \# \underline{[} \# \underline{[} \underline{]} \underline{]} \# \underline{[} \# \\ \underline{\text{N}} \text{ DET } \text{PRO} & \text{PRO } \text{DET } \underline{\text{N}} \underline{\text{N}} & \underline{\text{N}} \underline{\text{N}} \underline{\text{N}} \text{ PP } \underline{\text{V}} \text{S } \underline{\text{S}} \end{array}$

BC I and BC II, together, allow a three-way junctural contrast. At any given spot in a terminal string preceding or following all the segments of a word, we will either find ##, #, or no word boundaries at all. As I prove in Part III, it transpires that one word boundary separates two adjacent words a and b dominated by immediately pre-terminal nodes A and B if and only if either A or B (but not both) belongs to a non-lexical category and is immediately dominated by a node dominating the other. If both belong to non-lexical categories and every node dominating one dominates the other, no word boundaries show up between a and b. In all other situations, a and b are separated by two word boundaries.

Point one is that the specific SPE boundary conventions turn out to be inappropriate in individual cases--they en-

code in the terminal string the wrong pre-terminal information. We could fix them up if we wanted to, once we found out what the right pre-terminal information was. On the assumption that phonological rules are sensitive only to terminal symbols, we presumably want to.

Point two is that this assumption is unnecessary and in fact leads to complications. There is a list of what look to be universal juncture effects--predictable blockages of high-level phonological rules in certain structural configurations. Assuming that we can recast these structural configurations in boundary-theoretic terms and then refer to the boundaries in rules, it ought to puzzle us that these juncture effects do not vary universally, since a boundary is supposed to be a phonological unit which can be parenthesized or omitted, like any other unit, in a structural description.

Putting the matter somewhat differently, there are two separate questions.

(i) Does boundary theory as presently constituted express the aspects of tree geometry which are really and truly relevant to the proper functioning of phonological rules?

(ii) To what extent are these genuinely relevant factors to be regarded as contextually specifiable, and to what extent can we deposit them in universal grammar?

The answer to (i) is no, and I demonstrate it below by example. The answer to (ii) is that, to a surprisingly great extent, boundary effects in phonology are predictable

from the form of each rule, together with a universal statement.

An analogy might be apropos. Suppose someone invented a formalism whereby the A-over-A condition could be written into every transformational rule subject to it. Clearly this new capability would be a step backward, since it would imply that A-over-A effects are not universal. Pointing to rules which appear to counterexemplify A-over-A is a fairly feeble defense--on independent grounds we know that such exceptions (rules of anaphora and so forth) belong to components of rules which we do not expect to be subject to structural constraints on string operations in the first place.

I'm suggesting that, since juncture effects in phonology are largely predictable, it is a bad idea to invent a system of contextual symbols to stipulate them rule by rule.

In a similar way, having installed terminal boundary symbols looking very much like (and in fact being, in SPE theory) any other phonological units, we are tempted to try to manipulate them as we must the others. Such operations, and the phenomena requiring them, turn out to be (as far as anyone knows) prohibited and non-occurring, respectively. There are some famous counterexamples, like French liaison. I'll deal with these in the text.

Now of course it will be possible to lay substantive constraints on the use of boundaries such that these abuses will be ruled out. And there will be a boundary theory

which ferries to the terminal string exactly the right and universally useful pre-terminal information. Such a constrained boundary theory will merely be an imperspicuous way of saying that the pre-terminal information itself conditions rules. Which is my point.

In Part I I examine, with reference to English, blockage of phonological rules at (i) trace, (ii) parenthesis, and (iii) the end of a sentence. Take, for example, monosyllabic destressing.

(3) \*You're not as sick of your thesis as I'm t of mine.

(4) \*I'm, they tell me, cute.

(5) \*I think, therefore I'm.

There is a structural generalization uniting these blockages. The local phrase marker at each blockage site has a single geometric property, which I won't spill here. Let us assume for the time being that it exists. We then expect to find implicational clustering of effects analogous to those exemplified in (3)-(5) wherever any one is found. This is actually so, as I demonstrate in case studies of English (Part I) and Irish (Part II).

An interesting counterexample is discussed in Part IV. Part IV is devoted entirely to a rather poorly understood rule of English phonology, final dental palatalization before word-initial yod: coultjl you and the like. Among other interesting properties, this rule does not fail where monosyllabic destressing does.



(6) John want[š] your body more than he doe[ž] your mind.

(7) Thi[š], you see, seems to be quite reasonable  
English.

(8) Try i[č], you won't die.

What I show is that this fact is, at a certain level of analysis, to be expected. Palatalization stands to monosyllabic destressing with respect to juncture effects as, say, nominalization stands to WH-movement with respect to A-over-A: In a sense its exceptional status, since independently predictable, actually supports the theory it apparently counter-exemplifies.

As for the truly contextual function of boundaries, note that boundary theory presupposes a well-articulated theory of linguistically significant units. For every linguistically significant unit and level of phonological rules indigenous to it, boundary theory has a corresponding terminal symbol to demarcate it. Suitably elaborated, this level theory is, according to me, just about all we need to talk about in the way of contextual specification in the ordinary cases.

(In Part III, it becomes necessary to say that there is a unit which has no correlate in standard theory. This is the clitic group. By this means I wish to express the familiar but still curious status of clitics as, in various senses, intermediate between affixes and independent words.)

There are extraordinary cases. Many reasonably well-documented examples of rules of phrase-level phonology exist which have the property of "selective" application. This

means that they are sensitive, in obscure ways, to genuinely contextual non-phonological information which level theory alone is not rich enough to provide. Boundaries, in case after case, turn out to be inappropriate. I believe that it is possible to say reasonably strong things about the universal possibilities for non-phonological conditioning of phonological rules, and have them appear to stick. In Part III I propose what seems to me, with my experience with selective sandhi, to be the appropriate theoretical vocabulary making such rules expressible and the non-occurring ones inexpressible. The key notion is one of constituency, and it develops that normal level theory, which I also sketch, is actually a special case of it. Case studies: French liaison, Hebrew accentuation, Italian raddoppiamento.

The pioneer in this area is, of course, Lisa Selkirk, and I would like to publicly avow my admiration for and indebtedness to her and her work.

I make very few theoretical assumptions of especially controversial status in what follows. The ones I make are mainly of convenience, rather than of necessity. In order to have reasonably precise syntactic groundwork, for instance, I make use throughout of  $\bar{X}$  theory, the only existing account of phrase structure which says much. Bar theory is spelled out most comprehensively in Jackendoff (1977). In principle, empirical consequences of this decision could arise, particularly in connection with the selective processes of Part III. In fact, they do not. My point is only that the reader's

opinion of this easy assumption and similar ones should not interfere with my larger case. Pretty broadly and unequivocally, though, I accept most of the basic notions of standard generative transformational theory.

## PART I

## AGAINST BOUNDARIES

Before I say anything, I note the naked fact that phonological and morphological rules have idiosyncratic domains of application: The morpheme, the syllable, the word, possibly the phrase, and the sentence come to mind. Rules of phonology and morphology are not very different from other kinds in this respect; syntactic and semantic rules of course show analogous boundedness, proving themselves to be limited to certain phrasal categories, sentences, utterances, or discourses. In syntax and semantics, this unavoidable observation is perceived as an incitement to discover properties of the various rules, or, better, of the various components to which they belong, from which their limitations will follow. A fatalistic and slightly empty solution to the problem, which no one even thinks to propose, would be to set up ad hoc boundary symbols flanking each sort of domain, which our rules can now pay attention to as it appears necessary. By doing merely this we seem to be condemning ourselves to a lack of insight into the several systems of rules.

This same solution, however, is precisely the one generally accepted among phonologists. In order to implement it, as will become increasingly evident in the following pages, one quickly finds the need for a great deal of theoretical machinery to place boundaries, to delete most of them when

they pile up, and to ignore the rest of them when they get in the way. All of this comes from assuming that boundaries exist as items of vocabulary on a par with the others.

How, in practice, are boundaries actually referred to in rules? They are generally very handy in demarcating margins of domains of application. It is extraordinarily nice to be able to write \_\_\$, \_\_+, \_\_#, possibly \_\_##, and the mirror images. Recognition of this fact provoked the first appearance of real juncture symbols (Sapir and Swadesh 1939, Trager and Bloch 1941). It is not nearly as clear that reference to internal boundaries is necessary, i.e. that there are structural descriptions of the form

(1) ... [ +F ] B [ +G ] ...

, with specified terminal material on either side of a specified boundary B. Where B = ## or greater, (1) is of course utterly unexemplified. I offer most of the remainder of this work against the possibility of B = #. Examples of (1) where B = \$ are strikingly absent, to my knowledge. As for B = +, an apparent problem, most of the examples known to me involve allomorphy cooccurring with the presence of specific affixes preceding or following the alleged boundary. It seems to me that an easy and sensible thing to say for such cases is that the alternations are conditioned by the affixes themselves, rather than by the boundaries, whose presence is in turn conditioned by the affixes.

In any case, as Kiparsky (1974) demonstrates, his alternation condition, according to which neutralization processes

apply only to derived forms--i.e. only where an alternating form (but see Mascaro 1976) reveals the un-neutralized segment--predicts the presence in many rules of what's usually written +. In other words, it lets us get away with not writing it in, often simplifying matters considerably. Kiparsky cites, for instance, trisyllabic laxing, which has hundreds of exceptions: nightingale, overture, Rotenberg, etc. Since these don't feel very exceptional, it is not a nice solution to mark them as such. In fact all non-derived words fail to incur trisyllabic laxing. But writing in a required morpheme boundary is, by anyone's standards, a really ugly alternative, since a complex disjunction of them is actually necessary,

$$(2) \quad \text{---} \langle + \rangle_a C_0 \langle + \rangle_b V \langle + \rangle_c C_0 \langle + \rangle_d V \quad C_0$$

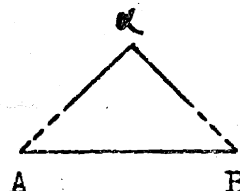
Condition: a or b or c or d

as examples like (a) anti+pathy, (b) san+ity, (c) penal+ty, and (d) omin+ous show. The problem, along with the boundaries, disappears given the alternation condition.

Now if it is true that essential internal boundaries are not found, we might bother to ask ourselves why not. Let us imagine that there are separable components of syllabic rules, morpheme-level rules, word-level rules, phrase-level rules, and sentence-level rules, each of which has the obvious domain of application, henceforth often abbreviated  $\sigma$ , m, f, w, and S. Clearly we need to be able to represent the notion edge of domain in our rules in one way or another.

Let me use the notations  $\_\emptyset, \emptyset\_\_$ , meaning 'at right edge', 'at left edge', interpreted identically for each component L of rules.

(3)



$\alpha$ , A, and B are nodes, A and B terminal ones. A and B are ultimately dominated by  $\alpha$ . Nothing to the left of A or to the right of B is dominated by  $\alpha$ . Whether  $\alpha = \sigma, m, w, f$ , or S, to say that a rule is restricted to  $\alpha$  is to say that its effects and conditioning are limited to the string consisting of A, B, and everything in between, written  $\overline{AB}$ . The left edge of domain  $\alpha$  is at A. The right edge of domain  $\alpha$  is at B.

Thus, in a word-level rule,  $\_\emptyset$  is equivalent to the usual  $\_\#$ . Etc.

Note, though, that  $\emptyset$  is not a boundary. It is just an extension of the usual 'null' notation, given sense now that domains end at the left and right. One can't perform operations on it. It would be absurd to delete it, for instance, just as it would be to try to delete the focus bar.

Without boundaries as terminal symbols, it follows more generally that there will be no "readjustment rules" (SPE, pp. 371 ff.) with the power to insert them, delete them, or shuffle them around in various ways. Such rules have rarely been proposed; see Part III below. These few examples aside, the fact is that boundaries, if phonological units, behave

remarkably unlike other phonological units, serving in rules only in contextual capacities. The same observation is made in Pyle (1972): It is probably not necessary to put a boundary on either side of an arrow in a phonological rule. If there are no boundaries, it is not even possible.

If we banish boundaries, it also follows that rules with the effect of (1) will be nonexistent. A morphological rule whose environment is  $\_\_\_ \emptyset [+F]$  is already senseless, because the presence of specified material to the right of the morphological edge contradicts the assumption that the rule is a morphological one. Similarly for the other types of rules.

One thing that falls out of this theory without coaxing is a universal implication to the effect that a rule which is strong enough to breach an edge at level  $n$  will also breach edges at the finer levels of structure. It is usually very easy to set up boundary-strength hierarchies saying exactly this. (See, for instance, Stanley (1969), McCawley (1965), Ramanujan (1967), Kenstowicz and Kisseberth (1977).) Why is it so easy? The idea of boundary "strength" is not quite circular. To say, e.g., that  $\#$  is "stronger" than  $+$  means a little more than merely that (generally speaking) any rule that applies across  $\#$  applies across  $+$ , even though, in a theory which uses boundaries, we could just as well say that  $+$  is stronger than  $\#$ : Notice that every word uniquely contains at least one morpheme.

In fact this is true of linguistically significant units



in general. If unit  $\varphi$  uniquely contains at least one unit  $\psi$ , a  $\varphi$ -boundary is "stronger" than a  $\psi$ -boundary. Every morpheme uniquely contains at least one segment, every word at least one morpheme, every phrase at least one word, and every sentence at least one phrase.

The syllable does not possess such implicational virtues. Neither a segment (obviously), a morpheme (-Z), nor a word ('d) need uniquely contain a syllable, though a sentence, it seems to me, must. But it does not follow from this last that every syllable must uniquely contain at least one word, or one morpheme, or even (due to ambisyllabicity, on which see Kahn (1976), or else below, and against which Selkirk (to appear)) one segment.

My point is that the syllable lies outside the containment hierarchy. Interestingly, it also lies outside the strength hierarchy. There are structurally-conditioned rules, like French liaison (below), which apply across junctures of one sort or another, but fail at syllable edge, just as across zero. (Why liaison does not apply across zero I'll give an account of in Part III.) A syllable, it appears, does not count as a structural unit in the same way as the other structural units.

There is a third respect in which the syllable differs from other units, this one, I believe, at the root of the other two. A syllable has no status at all in the lexicon--it is presumably established by rule in the phonology.

There are powerful indications that syllable construction

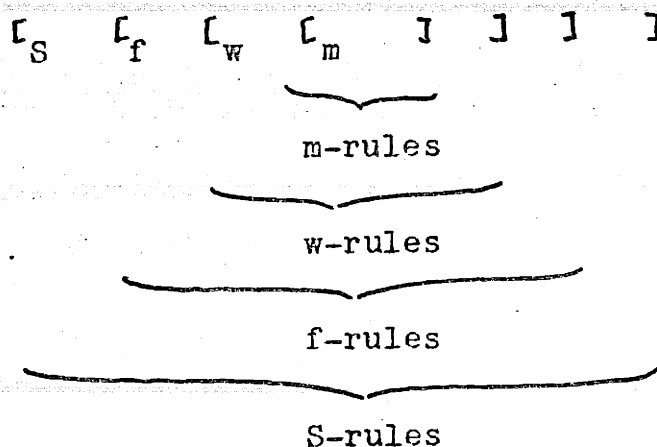
à la Kahn (1976) follows most phonological rules, indeed preceding only the syllabic ones. Note that, unlike true lexical units, the syllable is entirely dissociated from meaning. Note that there are no syllables which have to be marked as exceptions to otherwise valid generalizations. Note that syllables are not borrowed or lost.

I assume, then, that the syllable exists only outside the lexicon, of which hierarchical organization is a property in reasonably good standing. Supposing that it gets built quite late, after rules at the other levels, the syllable's status as a non-entity with respect to rules at them is explained.

A little more generally, it is useful and probably necessary to recognize an independent hierarchy of prosodic units, including the syllable, the foot, the stress group, etc. These, one presumes, bear strict containment relations to one another, but none to any member of the lexical hierarchy. (Morris Halle suggests to me that the segment may have dual membership, occupying the lowest position in both lexical and prosodic hierarchies.)

Restricting our attention to lexical units, the strength and containment rankings mentioned above are automatically related in this theory, and it follows that the syllable's exclusion from the latter should entail its exclusion from the former. An S-level rule, for instance, is one applying within  $[S]$ . By definition its operation includes the

lesser domains f, w, m.



Note that speaking of "boundaries" #, †, at the edges of these internal domains makes it possible to refer to them in the structural descriptions of high-level rules. This pseudo-possibility is what gives rise to the notation (B) and to implicational strength hierarchies to eliminate it. But their import is an automatic consequence of the nonexistence of boundaries.

Likewise an f-level rule is one applying within [f ].  
Etc.

Generally, it follows from the fact that R is a  $\psi$ -level rule that R will apply anywhere inside a  $\psi$ . There are many sorts of extenuating circumstances blunting the force of this implication. For instance, in Irish (Part II, below), the initial mutations, affecting the leftmost segments of words, are triggered by what appear to be word-sized abstract features. So, of course, the mutations will not be found applying across edges smaller than w, since the flanking units

will never bear the instigating features.

Opposed to the notion edge is the notion junction. Where the relation is-an-edge-of is unary, the relation is-a-juncture-between is binary; there will be no domain-final or domain-initial juncture. This is intended to correspond to the observation that there are rules at a given level L which involve only pairs of adjacent terminal symbols, and not the L-last one alone or the L-first one alone. Prime terminal elements p and q are adjacent with respect to some level of representation L if and only if p and q are productions of the rules of L and no r also produced by the rules of L falls between p and q in the terminal string containing them. As we will see, mere stringwise adjacency

(4)     ...     p     q     ...  
               [+F]   [+G]

does not guarantee that a rule operating at L whose structural description appears as

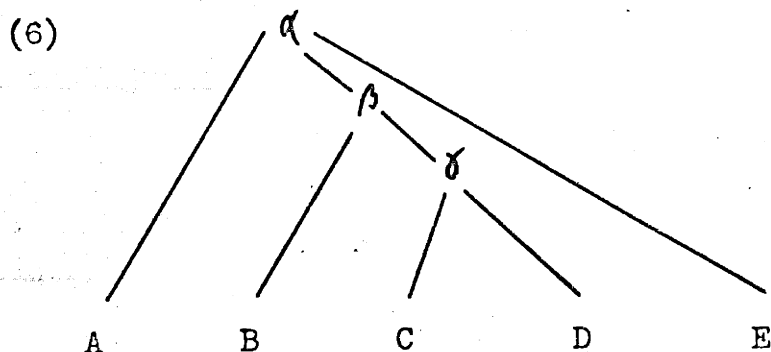
(5)     ...   [+F]   [+G]   ...

will apply.

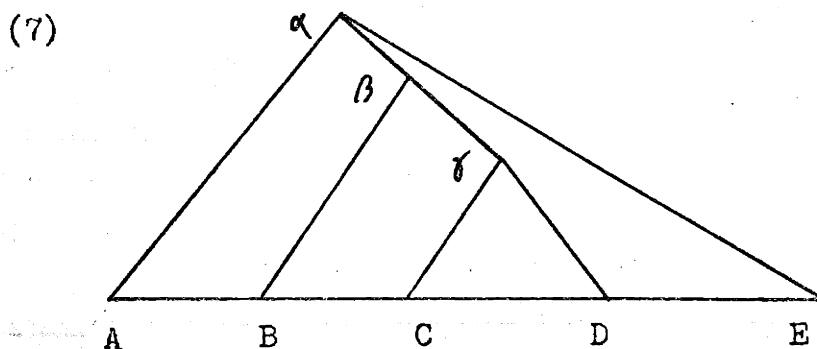
I'd like to say that there is such a thing as a normal juncture, in essence a locally well-formed tree, whose presence the application of rules of a certain independently justifiable type requires. These rules respect normality of juncture by their very nature, unlike edge of domain, which is genuinely contextual. That is, we know right away from the form of a rule that it will fail across an abnormal juncture, an object which we will soon encounter. A normal

juncture is not, I would say, the same thing as any known property of the terminal string (I'm talking about boundaries), as will become less unobvious below.

Defining the proper notion of juncture involves taking trees rather seriously as geometric objects. Look, for instance, at this one.



$\alpha - \delta$ , A - E are vocabulary provided by the rules of some level of structure L.  $\alpha$ , let's say, is the root of the tree. A - E are prime terminal lexical units U. Let us consider these nodes as points. Now we will complete (6) by drawing  $\overline{AE}$ , containing the terminal string.



We'll say that a U-juncture is the empty interior of a simple closed curve whose  $n$  vertices are from  $\{V_T^L \cup V_N^L\}$ ,  $n-1$  of whose sides are the lines of the usual L-marker, and

whose  $n$ th side contains the terminal string. Since a U-juncture is empty, exactly two adjacent members of  $V_T^L$ , U-units, are among the vertices of the curve bounding it. In fact a U-juncture is just a minimal area bounded by the lines of an L-marker. The four defined by (7) are (left to right) the interiors of:

- (8) (i)  $\ast AB \beta$   
 (ii)  $\beta BC \gamma$   
 (iii)  $\gamma CD$   
 (iv)  $\ast \beta \gamma DE$

The exterior of the large triangle  $\ast AE$  is not a U-juncture, because it is not the interior of anything. The interior of  $\ast AE$  is not a U-juncture, because it is not empty. Notice that every consecutive pair  $(i, i+1)$  of terminal elements  $1, 2, \dots, n$  determines a segment  $\overline{i \ i+1}$  which is one side of the curve bounding a different U-juncture: In (8),  $\overline{AB}$ ,  $\overline{BC}$ ,  $\overline{CD}$ ,  $\overline{DE}$  (the only roman letters in each example) are sides of (i), (ii), (iii), and (iv), respectively, and only them, respectively. Notice also that there is no U-juncture to the left of A or to the right of E. (Since U is a (prime) terminal element, there are no f-junctures--phrasal nodes are not terminal. Prosodic units, too, do not come with U-junctures.)

We might ask if there can exist a pair of consecutive units  $(U_i, U_{i+1})$  such that no curve containing them bounds a U-juncture. It is very convenient to say that there can, and that rules at level L will not involve both  $U_i$  and  $U_{i+1}$  in such circumstances.  $U_i$  and  $U_{i+1}$  will then meet at the

"abnormal" juncture which I alluded to.

I will often have occasion below to speak loosely of the U-juncture "between"  $U_1$  and  $U_{i+1}$ , or of application "across" U-juncture.

Let me restrict my attention to high-level phonological and morphological rules. (By "high-level morphology" I understand local morphological alternations conditioned by factors in the signal at large. Examples will come up.) Here L is S, syntactic structure at the level of the sentence; our trees are standard phrase markers. Thus we're talking about w-junctures, since words are the prime terminal elements here.

Let us assume that the rules in question require the presence of normal w-junctures. If any constant terms of the structural description of such a rule are satisfied by material belonging to two consecutive members of  $V_T$  not on the curve bounding any one w-juncture, the rule will block. An "abnormal" juncture, then, cannot be breached by rule.

The first case I want to consider is the English a/an alternation. First, though, I'm going to digress at length, delivering a number of slightly desultory remarks on some problems connected with the rule responsible, revolving mainly around its proper form and direction. These will ultimately turn out to be important to the larger discussion. The first remark concerns the various forms, from the twelfth century to the present, of the synchronic rule evidently behind this alternation.

Even in its modern form, the a/an rule is usually sup-

posed to be taking an to a, and is also usually supposed to have steadily generalized in applicability over the years. It is true that an is found in progressively diminishing numbers of environments, but this is an entirely different thing from saying that some rule is applying more. Actually writing a rule representing in itself each of the several intermediate stages is not a pleasant business, as we shall see. I'd like to suggest that these rules are in fact a good deal simpler than they appear. Various independent processes, it turns out, conspire to feed each a/an rule (or bleed it, depending on its directionality--more on this below), accounting in large part for its apparent complexity, while the rule's historical evolution consists mainly in coming to be couched in terms of the feature [±syll] instead of the feature [±cons].

OE had no indefinite article until around 1150, when the numeral án 'one' began to discharge that office. In its capacity as article, án became "proclitic and toneless" (OED) while the numeral retained its length and wound up going through the vowel shift. Preconsonantal a is found in midlands dialects from the beginning, as soon as the non-numerical sense developed. Still, an occurs preconsonantly through 1300 or so. An before [w] disappeared early, though before [y] (an yere, etc.) it remained until the fifteenth century, sometimes even later.

(9) 1611 Bible Acts vii 27    An eunuch of great authority.

(Such an one is an isolated archaism, found very late.) Even



after the fifteenth century, an occurs before unstressed [yùw]: an eulogium, an united appeal.

(10) 1823 Lingard History of England VI 219 An eulogium on his talents.

(11) 1847 Tennyson The Princess i 149 All wild to found an University For maidens.

Evidently there are still speakers who have an before [yùw].

OED records an before h (and possibly [h]), even beginning a stressed syllable, as late as the eighteenth century: An house, an heifer.

(12) 1611 Bible Acts vii 47 But Solomon built him an house.

(13) 1732 Pope Essay on Man iv 78 Nor in an hermitage set.

An before unstressed [hV] is rather hardy. An historic moment, an heretical proposal still have their utterers, although most speakers, and nearly all American speakers (I'd say), have no an before [h].

Thus we can bet on at least the following stages in the history of the alternation. (To the left of each slash is the post-a environment; to the right, the complementary one. V = vowel, C = consonant, G = non-glottal glide.)

- (14) (i) C / V, G, h
- (ii) C, w / V, y, h
- (iii) C, G / V, yùw, h
- (iv) C, G, hV / V, yùw, hV
- (v) C, G, h / V

We might, in a straightforward manner, attempt to write a rule for each stage. In that case we would be committed to rules (15)(i-v) below. (In order to avoid question-begging, I have written first the environment of an an  $\rightarrow$  a rule, then the environment of an a  $\rightarrow$  an rule. Having them both to look at will come in handy later.)

(15) (i) — [ +cons ]

(ii) —  $\left[ \begin{array}{c} -\text{syll} \\ -\text{cons} \\ \langle +\text{back} \rangle \end{array} \right]$

(iii) —  $\left[ \begin{array}{c} -\text{syll} \\ -\text{low} \\ -\text{cons} \\ \langle \text{a-back} \rangle \end{array} \right] \left\langle \begin{array}{c} \langle [+high] \rangle \\ \text{b} \langle [+back] \rangle \end{array} \right\rangle \backslash$   
 $\left\langle \begin{array}{c} \text{a} \langle \text{b} [+stress] \rangle \end{array} \right\rangle /$

(iv) —  $\left[ \begin{array}{c} -\text{syll} \\ \langle \text{a+low} \rangle \\ -\text{back} \\ \langle \text{b-cons} \rangle \end{array} \right] \left\langle \begin{array}{c} \langle [+high] \rangle \\ \text{c} \langle [+back] \rangle \end{array} \right\rangle$   
 $\left\langle \begin{array}{c} \text{a,c} \langle [+stress] \rangle \end{array} \right\rangle \text{b}$

(v) — [ -syll ]

(i)' — [ -cons ]

(ii)' —  $\left[ \begin{array}{c} -\text{cons} \\ -\text{syll} \\ \langle -\text{back} \rangle \end{array} \right]$

(iii)' —  $\left[ \begin{array}{c} -\text{cons} \\ \langle -\text{syll} \rangle \\ -\text{back} \\ \langle \text{a+high} \rangle \end{array} \right] \left\langle \begin{array}{c} [+high] \\ +\text{back} \\ \text{a} \langle -\text{stress} \rangle \end{array} \right\rangle$

(iv)' —  $\left[ \begin{array}{c} -\text{cons} \\ -\text{syll} \\ \langle \text{b} \langle \text{a-back} \rangle \rangle \end{array} \right] \left\langle \begin{array}{c} [+high] \\ +\text{back} \\ \text{a} \langle \text{b-stress} \rangle \end{array} \right\rangle$

(v)' — [ +syll ]

These are merely literal representations of the environments more perspicuously stated in (14), as the reader may verify if she really wants to. What I have just done nicely obscures, I think, the real nature of these rules. Let us first consider the variable operation of (15)(iv) before h. In most dialects [h] is somewhat lenited, at least, in exactly the environment \_\_\_\_V. It is very easy to suppose that initial h-deletion accompanies and feeds the a/an rule. Thus an will appear only to the extent that [h] disappears. It is an important fact, to my mind, that an before (written, anyway) h is found especially in British dialects, among exactly which h-deletion has real documentation. In the dialects where initial [h] is entirely gone, whether by deletion or relexicalization, an is always present before orthographic h; see Sivertsen (1960), Orton and Dieth (1962). It is tempting to make similar conjectures even about some of the writers contributing to stage (iii): Initial [h] was always gone, so an was always present. This last obviously requires research.

To give an analogous example, speakers of some dialects of Australian English regularly and unilaterally palatalize dentals before yod, intervening "boundaries" notwithstanding. (More on this in Part IV.) Thus we hear:

- (16) thi[<sup>ʲ</sup>s] yo-yo
- (17) tho[<sup>ʲ</sup>z] yellow balloons
- (18) fluen[<sup>ʲ</sup>c] Yiddish
- (19) my thir[<sup>ʲ</sup>] urologist

The unpalatalized versions are ungrammatical in connected speech. However, the word and behaves strangely: an[ʃ] results only optionally. It develops, of course, that [ʌnd] alternates on a sporadic basis with [ʌn]. In the same way, jus[č] you is found only as often as ju[š] you, simply because [ʃʌst] alternates freely with [ʃʌs]. I'm suggesting that sporadic h-deletion correlates with sporadic an in a similar fashion.

It is a disturbing thought that, in this otherwise reasonable account, the morphological rule relating an and a must follow the surely crudely phonetic rule of h-deletion. This problem I'll remedy below.

Next, let us look at the problem of an before [yùw]. Why is an united appeal so different, even to the ear of someone who does not speak this way, from \*an unified theory, still more from \*an Yàstrzemiński fan, and especially from \*an wòn ton recipe? Before I answer this question, I would like to point out the curious fact that while many speakers of English have words whose initial segments are [nyù-] (neurotic, Newtonian), and some have initial sequences in [nyú-] (new, neuron, Newton), there are no English words in [nyV-], where V is not [u], or in [nw-].

My idea is that an  $\varphi$  is grammatical, where  $\varphi$  is a permissible initial sequence, to the extent that n $\varphi$  is also a permissible initial sequence. That this should be so follows from the theory of Kahn (1976), together with the assumption that a is the underlying form of the article, in



To the extent, small or nil for most people, that initial [ny-] is "permissible" (presumably at the surface), IIa gives

$$(21) \quad \begin{array}{c} \text{ə n y } \dot{\text{u}} \text{ w...} \\ \swarrow \quad \searrow \quad \swarrow \\ \sigma \quad \sigma \end{array}$$

If [ny-] is not good, IIa does not allow urologist to appropriate an initial n. By IIb we end up with

$$(22) \quad \begin{array}{c} \text{ə n y } \dot{\text{u}} \text{ w...} \\ \swarrow \quad \searrow \quad \swarrow \\ \sigma \quad \sigma \end{array}$$

Kahn also has a rule III, applying only in "normal-rate and faster speech":

$$\text{III In } [-\text{cons}] \text{ C C}_0 \left[ \begin{array}{c} \text{V} \\ \text{--str} \end{array} \right]$$

$$\begin{array}{c} | \quad \swarrow \quad \searrow \\ \sigma_1 \quad \sigma_2 \end{array}$$

associate C and  $\sigma_1$ .

This rule is designed to account for the ambisyllabicity of, for instance, the n in money in reasonably un-deliberate speech.

$$(23) \quad \begin{array}{c} \text{m } \wedge \text{ n i} \\ \swarrow \quad \searrow \\ \sigma \quad \sigma \end{array} \rightarrow \begin{array}{c} \text{m } \wedge \text{ n i} \\ \swarrow \quad \searrow \quad \swarrow \quad \searrow \\ \sigma \quad \sigma \end{array}$$

From (21), then, III gives (24),

$$(24) \quad \begin{array}{c} \text{ə n y } \dot{\text{u}} \text{ w...} \\ \swarrow \quad \searrow \quad \swarrow \\ \sigma \quad \sigma \end{array}$$

since the y is unstressed. (22) stays as it is. Now if we merely install a surface filter (25)

$$(25) \quad * \left[ \begin{array}{c} \text{ə n} \\ \text{DET } \swarrow \quad \searrow \\ \sigma \quad \text{x} \end{array} \right]$$

to the effect that the n of an must be ambisyllabic, we automatically reject (22), due ultimately to the fact that [nyuw-]



grounds of simplicity on which to base a choice of underlying form, nor are there in (15). On the other hand, recall that my idea above, at least in the modern dialects (iv) and (v), works only if a → an.

I'd like to call attention to the fact that (25) appears to be automatically rejecting all an C, without any help from (26). This is because, according to II, n is never ambisyllabic in such circumstances, while it always is before V. Unfortunately, the a/an alternation still happens when there is no possibility of syllabic association, as we will see below. Nevertheless, we might be able to get along with a rule of optional n-insertion stating merely

$$(27) \quad \emptyset \rightarrow n / a]_{\text{DET}} \text{ --- } [+seg]$$

dispensing with (26)(iv, v) completely, if we put on (25) the construction that syllabic non-association of n is out only for reasons of syllable composition, i.e. only if the x legislated against by (25) arises by Kahn's rules. As we shall see, the only kinds of extenuating circumstances allowed involve extralinguistic interruptions of the signal, which undoubtedly take place after (25) anyway. So now nothing remains of (26). (25) is quite general and accounts for the modern alternation all by itself, given (27). (27) is the unmarked rule of external sandhi--"an may occur before something." Why even this much is necessary will soon be clearer.

As we have seen and will see further, the problem of the



underlying form of the article, not in itself an especially fascinating question, interacts with weightier and more interesting matters. I should mention that a small debate (Hurford 1971, 1972, 1974; Vennemann 1972, 1974) raged some years ago on this very topic, Hurford championing underlying an and a synchronic recapitulation of diachrony, Vennemann pushing for underlying a and an inversion of the historical process. As nearly as I can make out, nothing more than a little bit suggestive is said in these papers. Each man admirably points out the defects in the other's case. Vennemann, for example, cites the generally earlier acquisition of a as evidence for its lexicalization in that form. Hurford sensibly questions the assumption that underlying forms are learned early and subsequently left alone. Vennemann mentions the modern English construction a whole nother story as indicating the underlying absence of the n. Hurford observes that this particular "false" division is completely idiosyncratic and nonproductive. Hurford and Vennemann force each other to admit the inconclusiveness of the evidence from ME false analysis, of which examples going both ways (an ewt > a newt, a nadder > an adder, an otch > a notch, a napron > an apron) exist. Hurford suggests that an n-deletion rule is "a more economical and natural rule than a rule specifically inserting an [n], which must, of course, be fully 'spelled out' in features by the rule." Vennemann professes ignorance of Hurford's conception of "naturalness," but observes that, given Hurford's assumptions, a full spell-out of the n must

be done somewhere, presumably in the lexicon, rendering any "economy" illusory. Finally, Vennemann notices that the a \_\_\_ environment is larger, in number of words, than the an \_\_\_ environment, and thinks that this would force lexicalization of a. Against this I might mention the example of liaison in French (see Part III), where final consonants are audible under very restricted phonological and syntactic circumstances; but the choice of the liaison consonant must clearly be made on a word-by-word lexical basis. There is no reason why underlying forms should not be set up on the strength of everyday but numerically comparatively scarce alternations.

I want to go on now and mention a few interesting facts which appear to lead again to the conclusion that n is inserted. The notion w-juncture is essential here.

It is important to realize, first, that the a/an rule is not phonological. Obviously it applies only to the one word. Further, it is not blocked by pause.

(28)(a) an...Eskimo Pie

(b) \*a...Eskimo Pie

(29)(a) \*an...snorkel

(b) a...snorkel

In fact, any kind of non-linguistic material, including coughs, sneezes, hesitation noises, throat-clearings, etc., is permissible in place of the three dots in (28) and (29). Notice, in the cases of the cough and the hesitation,

(30) an-- [ʔʔʔʔ]--Eskimo Pie

(31) a-- [ʔ:]--snorkel

that if the rule were really strictly local, we would get a before the [-syll] glottal stop and an before the [+syll] schwa. Presumably all this is so because the rule is morphological, hence comparatively early, and immune to such last-minute additions to the signal as pauses and other extralinguistic signs of physical and mental distress of various sorts. With this in mind, consider the parenthetical.

(32)(a) \*an--though I hate to admit it--silly idea

(b) \*an--though I hate to admit it--absurd idea

(33)(a) a--though I hate to admit it--silly idea

(b) ?a--though I hate to admit it--absurd idea

(34)(a) \*an--although I hate to admit it--silly idea

(b) ?an--although I hate to admit it--absurd idea

(35)(a) a--although I hate to admit it--silly idea

(b) a--although I hate to admit it--absurd idea

Judgments are certainly delicate, but it is curious that a is so good so often. Generally speaking, a (parenthetical)  $\bar{N}$  goes through, while an (parenthetical)  $\bar{N}$  fails. Whether a vowel or a consonant begins the parenthetical or the string that resumes to the right of it doesn't seem to matter.

(The reader who finds all such examples, with a parenthetical after an article, slightly precious to begin with will probably agree that including a kind of resumptive article

(36) a--unless I'm mistaken--a really distinct improvement

improves their naturalness markedly. The point I'll be making about the constituency of the parenthetical is unchanged.)

Parentheticals like those in (32)-(35) definitely belong in the representations of the larger strings. What we would like to see is a way of predicting this peculiar failure of the a/an rule from some property of theirs. The pre-parenthetical pause is irrelevant, as (28) and (29) show.

(My feeling about (33)(b) and (34)(b) is that sheer pedantry is responsible for the temptation to make the long-distance alternation, much like, say, the "proper" consistency of tenses throughout a discourse.)

I should first distinguish three or four classes of interpolated material, only one of which I will consistently refer to as parentheticals. First are the well-known subject-ency-transparent clauses, X say and the like (Erteschik 1973):

(37) Who did you say you were?

(38) \*Who did you repeat you were?

Note the inversion, the lack of pauses, and the smooth intonation, all of which assure us that this type of "parenthetical" is firmly engaged in the surrounding syntactic structure, though puzzling in familiar ways.

Second is the class of interpolated exclamations and various other expressions of spontaneous emotion.

(39) --hey!--

(40) --what was that?--

(41) --now cut that out!--

These have all the properties of interruptions. They niche (Ross 1972) absolutely anywhere, and have no semantic relation at all to the host sentence.

(42) Four--quiet in back!--score and seven years ago...

In place of quiet in back! we can again have things like throat-clearings, coughs, and so forth, making it a virtual certainty that interpolations like these are last-minute performance effects, having no structural relation at all to the sentences they interrupt. In addition to their syntactic and semantic isolation, they also fail to block sentence phonology involving things on either side,

(43) an--good lord, did you see that?--easy recipe unlike, and here I come to the class of parentheticals I will be considering,

(44) \*an, shall we say, easy recipe

Also compare (42) with (45):

(45) \*Four, I think, score and seven years ago...

Again unlike insertions of the good lord type, these are semantically related, in obscure ways, to their surroundings. Also, they both come with a special intonational contour and induce one on the outside sentence.

(46) This is, as you can see, an example sentence.

(47) This is--good lord!--an example sentence.

(48) This is an example sentence.

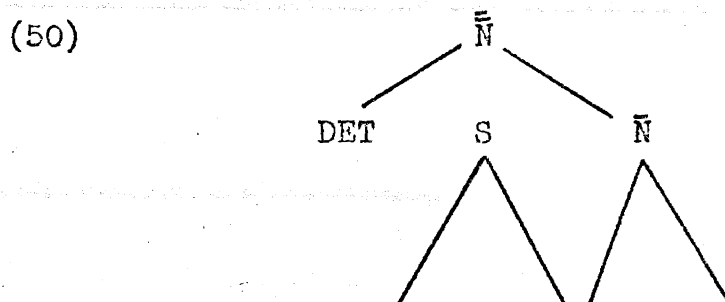
Good lord!, on the other hand, merely interrupts the outer contour.

Finally, the "true" parentheticals as you can see, etc., often do not stand alone.

- (49) \*I conjecture.  
 \*You'll appreciate.  
 \*We can say.  
 \*Though.

It certainly looks like the syntax must register their presence in some way, unlike the fairly trivial good lord cases.

The appropriate derived structure is an interesting problem. As nearly as I can make out, there are no reasons whatever for supposing that the parenthetical is dominated by anything in particular in the surrounding phrase-marker. Rather than make some arbitrary decision, I propose to give up entirely and let the parenthetical clause be geometrically independent of the rest of the sentence, sharing only its terminal string with it:



This has the advantage of predicting root phenomena in the parenthetical, which are in fact observable (see Emonds 1974):

- (51) a-- { don't you agree } --very important message  
           { don't forget now }

In addition, comma intonation (46) can now be given a struc-

tural basis.

We might also think of calling non-restrictive relative clauses parentheticals in this sense, thus accounting for their familiar intonational properties, which are parallel in every respect to those of the interpolated clauses we've been considering. Obvious but still puzzling contrasts like (52) versus (53)

(52)(a) The chutney which John gobbled

(b) The chutney that John gobbled

(53)(a) The chutney, which John gobbled

(b) \*The chutney, that John gobbled

turn out to be explained as well. The parenthetical is a root S and cannot begin in a complementizer any more than any other root S.

(54) \*That John gobbled the chutney.

But WH-movement can apply in both the (a) examples.

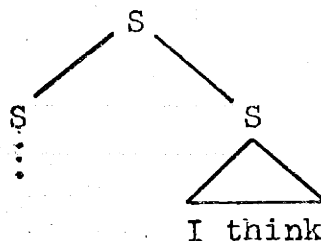
Emonds (1974) has suggested that internal parentheticals come by rule from sentence-final position. He wants to say that (55) and (56) below are related by rule

(55) John blames fluoridation, I think.

(56) John blames, I think, fluoridation.

because only then can he say that the parenthetical is at some point a root S (Emonds 1970).

(57)



This in turn he wants to say because root transformations actually do apply independently in the two clauses, as in (51) and (58).

(58) Does John blame fluoridation, do you think?

This idea seems really questionable, first because there are all sorts of non-sentential parentheticals, like however, in sum, without doubt, etc., which there is no special reason for getting from somewhere else. Notice too the following examples.

(59) John, however, blames fluoridation, I gather.

(60) \*John blames fluoridation, however, I gather.

It is also interesting that a parenthetical in an isolated NP is quite sensible, though there is no sentence for it to have been a right daughter of.

(61) a noun phrase, to take just one example, like this  
one

Generally, of course, moved constituents show up in sentences that also contain gaps vacated by them.

(62) \*Where John.

(63) John.

Probably the biggest advantage of a no-movement analysis is the movement rule it doesn't require. This rule seems to be excluded by any reasonably confining theory of transformations. Notice that deriving examples like (64)

(64) Everybody's gloating over the fact that Alonzo  
blindly accepts Alix's facetious claim that John,  
I think it is, and his sister blame fluoridation.

from a source like (57) involves violating nearly every con-

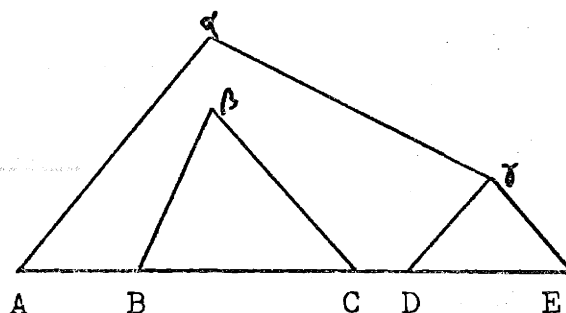


straint in the literature, whether one extraposes, in the manner of Emonds, and his sister blame fluoridation (a non-constituent), or intraposes I think it is.

Finally, the problem of the constituency of the parenthetical with respect to its surroundings, concerning which, as far as I know, there are not even plausibility arguments, is totally obviated. This issue, I should mention, is entirely distinct from the one about what sorts of constituents a parenthetical may appear between: This is the "niching" problem (Ross 1972) and it is unrelated.

Now if we assume that a is the underlying form of the article and that there is a rule of high-level morphology on the order of (27), (27) will predictably fail if the segment after the focus begins a parenthetical.

(65)



$\alpha AB \beta$  does not bound a w-juncture, because it is not closed.  
 $\alpha ABC \beta$  does not bound a w-juncture, because it is not closed either. Even if it were, its interior would not be empty.  
 $\alpha ABCD \gamma$  does not bound a w-juncture, because, even though it is closed, its interior is not empty. In fact there is no w-juncture bounded by any curve containing A and B. Thus a rule crucially involving terms analyzable as A and B, e.g.

(27), will not apply.

A second type of case is what is usually thought of as to-cliticization.

(66)(a) Who does John want t to speak?

(b) \*Who does John wanna speak?

(67)(a) Who does John want to speak to t?

(b) Who does John wanna speak to?

As is well-known, to-cliticization (and hence the reduction it feeds, for a small number of verbs) is blocked across an extraction site t. The rule looks maybe something like (68) (see Bresnan 1971, Selkirk 1972):

$$(68) \quad V \quad [_{VP} \quad \underline{to} \quad \rightarrow \quad [_{V} \quad V \quad \underline{to} \quad ]_{V} \quad [_{VP}$$

Actually, (68) is probably a little muscular. We might reasonably ask why it is necessary to adjoin--"cliticize"--to on the left at all. The usual thought (Selkirk 1972) is that a word belonging to a non-lexical category, e.g. to, may lose its stress and eventually reduce only if it is a "syntactic dependent," roughly equivalent to not being a phrase unto itself. (I return to this below.) Note, for instance, the behavior of the preposition to:

(69)(a) I'm talking ta you.

(b) \*Who are you talking ta?

But alongside of sentences like (67)(b), there are also sentences like (70), where to is presumably all that remains of a verb phrase.

(70) I don't wanna.

Hence it appears necessary to reattach to on the left, making

it once again dependent. Following this operation, to may lose its stress and reduce, and want may suffer its familiar phonological vagaries into the bargain.

Notice, though, that to fails to be a "syntactic dependent" precisely when something has been removed to the right of it. I believe that this is actually the salient feature of the phenomenon, and it falls easily out of the theory I'll elucidate directly. In any case, this exception to one side for the moment, there's very little in the way of saying simply that to can reduce anywhere.

But again, (70) is puzzling--it is an exception to the exception. After all, one can't say

(71) \*I don't intend ta.

which is parallel in every respect. We are forced to admit that want is exceptional. (68), incidentally, falsely denies this--it does not distinguish (70) from (71).

Suppose we said merely

(72) want to → wanna

as do Chomsky and Lasnik (to appear) (but see Postal and Pullum 1977). This minimal theory appears to give all the right results without awkward readjunctions. It embodies the observation that the list of contracting verbs is very short. Even when to reduces, it is only want that undergoes segmental alterations.

(73)(a) I meant ta finish.

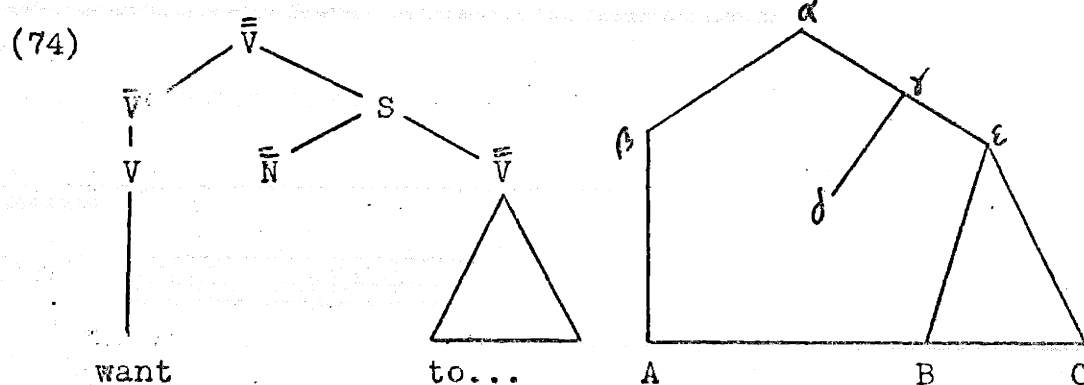
(b) \*I menna finish.

There are hafta, gonna, but it is not so clear to me that

these are not lexicalized modals, as much as must, will, though there are some distributional differences. Still, supposing they aren't, I see nothing wrong with a pair of rules like (72) producing them.

In my opinion, then, there is no to-cliticization. Thus it is (72) that fails mysteriously in (66). (It's not so clear, in fact, in what form cliticization rules are needed at all. See Part III.)

Given the now standard assumption that a trace is simply pre-terminal vocabulary dominating no terminal material, this blockage is not really mysterious.



$\alpha\beta A B \epsilon \gamma$  does not bound a w-juncture, because its interior contains  $\delta$ .  $\alpha\beta A \delta \gamma$  does not bound a w-juncture, because it is not closed. There is no w-juncture bounded by any curve containing both A and B. Therefore (72), whose two constant terms are want and to, will fail precisely here.

To instance a third rule,

(75) John  $\left\{ \begin{smallmatrix} \text{is} \\ \text{'s} \end{smallmatrix} \right\}$  more fed up with his thesis than his  
committee  $\left\{ \begin{smallmatrix} \text{is} \\ \text{'s} \end{smallmatrix} \right\}$  enthusiastic.

auxiliary contraction is of course possible in the first

clause, but not in the second. The literature on this phenomenon is large: See King (1970), Bresnan (1971), Baker and Brame (1972), Selkirk (1972), Lightfoot (1975). I assume, following pretty much Selkirk (1972), that contraction of is is fed by the more general monosyllabic destressing rule (pp. 42 ff.), which, incidentally, is also responsible for destressing to above.

(76)  $V \rightarrow [-\text{stress}] / [\# W [C_o \_\_ C_o] [(\#) X \acute{V} Y (\#)] Z \#]$

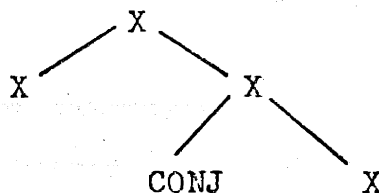
Condition:  $X \neq T \# \# U$

What (76) is supposed to say is simple: A monosyllabic dependent gets destressed. A "dependent" is a member of a non-lexical category ( $[C_o \_\_ C_o]$  in (76)) which is immediately dominated by a node also dominating a member of a lexical category. A lexical category is, of course, one participating in the bar system and coming, in SPE theory (see introduction), with flanking #'s. This lexical category, in (76), is in  $[(\#) X \acute{V} Y (\#)]$ , providing the condition, which ensures that no phrase boundary intervenes, is satisfied. Non-lexical categories are just those which commonly destress and reduce in ordinary speech: auxiliaries (will [l]), determiners (the [ðə]), prepositions (to [tə]), conjunctions (or [r]), and complementizers (that [ðət]). These are quite capable of bearing stress; but since stress requires unreduced vowels, the idea, doubtless correct, is to condition reduction on stresslessness by a rule like (76). The kinds of things accounted for by (76) are represented in (77) below. I instance once again to.

- (77)(a) Mary spoke roughly ta Jóhn.  
 (b) Mary spoke roughly ta th<sup>o</sup> thrée of them.  
 (c) Mary spoke roughly ta th<sup>o</sup> Pássamáquóddies.  
 (d) \*John was spoken roughly ta.  
 (e) \*John was spoken roughly ta by Máry.

Note that the non-lexical categories, which we might, following Chomsky (1970), classify as  $[-\frac{N}{V}]$ , already have the property that, by virtue of the very phrase structure of English, they are dependents, occurring to the left of their heads. (With respect to conjunctions, the one slightly dubious case, I accept Ross' (1967) account, according to which X-CONJ-X, X an arbitrary category, has the structure (78).)

(78)



When, then, do they fail to be dependents? When the head has been removed, as in (77)(d,e), (71). But this we have a way of dealing with.

It seems to me that (76) says no more or less than (79).

(79)  $V \rightarrow [-\text{stress}] / [ \begin{smallmatrix} [-N] \\ [-V] \end{smallmatrix} ]^{\text{C}_o} \text{---}^{\text{C}_o} ] [+seg]$

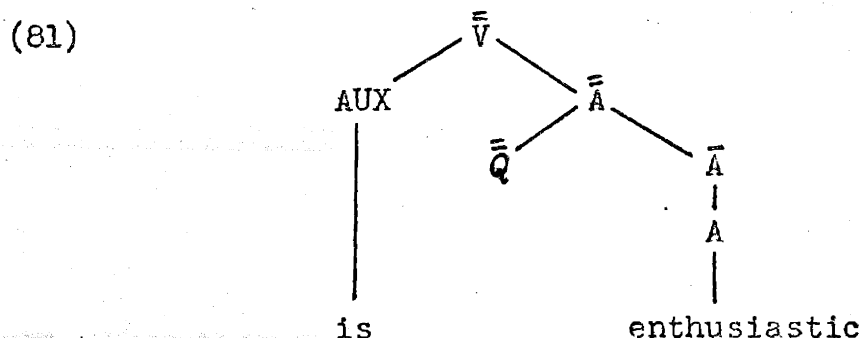
According to (79), a monosyllabic non-lexical category de-stresses before something. The stress condition in (76), incidentally, seems to be unnecessary:

(80) Mary spoke roughly ta m<sup>o</sup>.

Now (79), a rule of high-level phonology, ought to pat-

tern with the a/an rule (27) and the wanna rule (72) with respect to failure across abnormal junctures.

Recall (75). I assume that the pre-terminal debris of the extracted (I use a neutral term; see Bresnan (1975), Chomsky (1977)) quantifier in (75) is present in the phrase marker. The result is something like (81).



Then is and enthusiastic about no one w-juncture, and (79) fails as desired. Similarly preposition-stranding facts like (77)(d,e).

Unlike the other non-lexical categories, be does not seem to show up only as a left branch. There are examples like (82), in which be occurs at sentence edge.

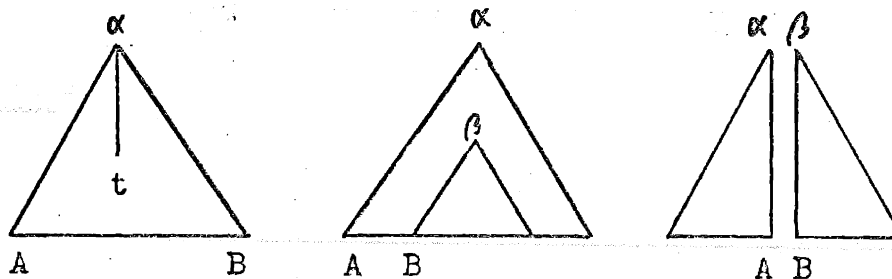
(82) I think, therefore I am.

Here, of course, I'm is impossible, even if more phonological material follows.

(83) \*I think, therefore I'm. Descartes said that.

We might at first think that this is as predicted. There are, to my knowledge, exactly three ways that a juncture can escape normality. They involve trace, parenthesis, and sentence edge, respectively.

(84)



In all three cases, A and B abut no one w-juncture. The first two have already been encountered. The third is hard to find examples of in English, because the relevant rules affect non-lexical categories, always left branches. And unfortunately, I don't believe that be is an exception. Be 'exist' simply never reduces, any more than will 'desire'. These are surely verbs.

The desired examples do occur in French, however. See Part III.

Interesting support for this analysis of auxiliary contraction comes from sentences like the following.

(85) \*John's, I'm told, shattered by the news.

A theory stating simply that trace blocks auxiliary contraction has nothing to say about (85), where (one hopes) no extraction has happened. But if the monosyllabic destressing rule (79), and hence auxiliary contraction, respects w-juncture in the same way as the a/an rule (27), failure of (79) across parenthesis will parallel that of (27).

(In both cases, I reiterate, the pre-parenthetical pause is irrelevant:

(86) an...absurd idea

(87) John's...completely shattered



These are perfectly decent, with no special limit on the number of dots. Once again, all manner of sound effects, hemming, hawing, throat-clearing, and so forth--anything without linguistic significance--are permissible during the pauses of (86), (87). These presumably join the signal at the last minute and have no structural reflexes at all.)

It should also be true that, in general, destressing by (79) fails before a parenthetical--not just when destressing conditions contraction. This is also the case.

(88)(a) \*You {<sup>o</sup>[kʌn]}  
[kn̩]}, I hope, explain that away.

(b) You [kʌn̩], I hope, explain that away.

(c) You {<sup>o</sup>[kʌn]}  
[kn̩]} explain that away, I hope.

(89)(a) \*I'm [fr̩], unquestionably, wombat leash laws.

(b) I'm [fɔ̃r̩], unquestionably, wombat leash laws.

(c) I'm [fr̩] wombat leash laws, unquestionably.

(90)(a) \*I ran into Matthew, Mark, Luke, John, [n̩],  
surprisingly, Ralph, at the store.

(b) I ran into Matthew, Mark, Luke, John, [ɫənd̩],  
surprisingly, Ralph, at the store.

(c) I ran into Matthew, Mark, Luke, John, [n̩]  
Ralph, surprisingly, at the store.

The (a) sentences (of a type noticed first by Bresnan (1971)) seem quite operable upon by (79).

Again, the wanna rule does not apply either, though applicable, when the verb ends a parenthetical or when to be-

gins one:

(91)(a) \*I'll try, if you wanna give you the results tomorrow.

(b) \*I wanna tell the truth, one scoop of vanilla.

These are, of course, really terrible, even when a reasonably rapid delivery squeezes out all pauses.

The logically possible sixth case, blockage of the a/an rule by extraction, is contingently impossible by the rules of English: Extraction of a nominal obviously cannot get away with leaving an article behind.

I want to point out that the idea of normality of juncture is quite independent of anyone's idea of word boundary.

Suppose we accept the theory of Selkirk (1972), according to which a trace consists of word boundaries. In this sort of treatment, the structural description of each rule of trans-word-level phonology or morphology is written so as to permit the presence of at most a single # between its terms. Then it will fail across trace.

In connection with failure across parenthesis, we might have the reasonable suspicion that any  $\bar{S}$ , even a parenthetical one, will be flanked by ##. This in fact already follows from SPE conventions.

(92) a--##although I hate to admit it##--

Thus the a/an rule, written

(93) a  $\rightarrow$  an / \_\_\_\_ (#) V

obligingly fails where it ought to. The immediate objection is the existence of examples like the following.

- (94) (a) a--idiot that I am--  
 (b) a--in my opinion, anyway--  
 (c) a--amazingly--
- } totally unex-

pected result

We would have to say that all parenthesized strings, dominated by  $\bar{S}$  or not, are flanked by  $\#\#$ . Needless to say, this has a certain air of the ad hoc, although it could be true. If we found another environment where the rule failed, there would be nothing in particular to stop us from putting  $\#\#$  there too. The symbol  $\#\#$  has at this point descended to the level of diacritic, more or less without independent function or significance apart from saying, This is a spot across which rules are observed not to apply. A theory as weak as this (which has no adherents, to my knowledge) is not easy to prove wrong.

I think that the right question to ask here is whether a language learner could be counted on to have consistent exposure to sentences in which the three rules discussed in this Part have not applied across trace or parenthesis--i.e. whether one learns that each structural description contains ( $\#$ ), an ordinary contextual term. The answer is evidently no. Therefore it seems preferable to look for more general properties of these rules or the structures they apply to from which their behavior in such utterly marginal cases is predictable. I'm saying that ( $\#$ ) is not a contextual term--it simply has to be there. Yet a "boundary" is an item of

vocabulary which one can presumably refer to or not, as one chooses, like any other item of vocabulary.

It is telling that there are no rules applying across full stop. Why is it that there are no rules of discourse phonology, affecting the last segment of a sentence under the influence of the the first segment of the next, or vice versa? It follows from the theory I'm defending, since there are no domination relations among sentences in a discourse. But even supposing we set up outermost sentence boundaries ###, the absence of rules in whose structural descriptions we referred to (###) would be unexplained.

Returning to the parenthetical issue, it is worth pointing out examples like (95),

- |         |                     |                  |
|---------|---------------------|------------------|
| (95)(a) | an I can't tell you | } how silly idea |
| (b)     | an I don't know     |                  |
| (c)     | an it's incredible  |                  |

in which the pre-nominal  $\bar{A}$  contains a sentence, albeit of a distinctly limited type.

- |         |                                |                  |
|---------|--------------------------------|------------------|
| (96)(a) | *an I'm completely ignorant of | } how silly idea |
| (b)     | *a John doesn't know           |                  |
| (c)     | *an I know                     |                  |

The analysis we ought to give things like (95) presumably looks like (97).

- (97) an [ $\bar{A}$  [ $\bar{S}$  I can't tell you how [ $\bar{A}$  silly]]] idea

The constituency relations of the embedded sentence are slightly at issue. Several considerations go to show that a structure like (97) is appropriate. The most obvious is

that I can't tell you (how) is not a parenthetical.

(98)(a) \*a(n), I can't tell you, how silly idea

(b) \*a(n), I can't tell you how, silly idea

(c) a, you'll agree with me, silly idea

Further, I can't tell you and how behave like a constituent, moving (99)(b) and even appearing in the first place (c,d) together.

(99)(a) I can't tell you how silly an idea

(b) \*I can't tell you a how silly idea

(c) \*an I can't tell you very silly idea

(d) \*an I can't tell you silly idea

We'd surely like to connect these possibilities with those for the normal matrix I can't tell you:

(100)(a) I can't tell you how silly a person he is.

(b) \*I can't tell you a how silly person he is.

(c) \*I can't tell you really silly person he is.

(d) \*I can't tell you silly person he is.

The natural idea is to put how in the complement of I can't tell you in (99), just as in (100).

I can see very little reason for a boundary theorist to say that ## does not separate an from the next word in (95). However, (93) now gives (101):

(101)(a) \*a I can't tell you

(b) \*a I don't know

(c) \*a it's incredible

} how silly idea

(27), by contrast, gives the good results, since an and the next word about a single w-juncture.

Even more telling are examples like (102).

(102) a what they call adult game

Again, what they call is no parenthetical.

(103)(a) \*a, what they call, adult game

(b) a, what's the expression, adult game

The embedded sentence is surely inside the adjective phrase, standing to adult more or less as do really, especially, etc.

In fact, the syntactic possibilities for what they call X parallel those for the workaday matrix They call that X.

(104)(a) a what they call adult game

(b) what they call an adult game

(c) \*a what they call game

(d) \*a what they call very adult game

(e) what they call a very adult game

(105)(a) They call that adult.

(b) They call that an adult game.

(c) \*They call that game.

(d) \*They call that very adult game.

(e) They call that a very adult game.

We could account for this by embedding (e.g.) precisely (105)

(a) in the source for (104)(a).

(106) a  $[\bar{A} [\bar{S} \text{ they call what } [\bar{A} \text{ adult}]]]$  game

the necessary derivation by WH-movement shows that the embedded sentence is  $\bar{S}$ , not just S, as is at least possible in (97). No WH-words are vowel-initial. But there are almost identical examples like (107)

(107) an as I guess they call those things adult game

where the vowel-initial complementizer shows up. We do not have a.

The conclusion which this is helping us to draw is that word boundaries are not only dispensible, but actually incorrect.

With respect to auxiliary contraction through monosyllabic destressing, analogous examples suggest themselves.

(108) John's  $\left\{ \begin{array}{l} \text{I can't tell you} \\ \text{I don't know} \\ \text{it's absolutely incredible} \end{array} \right\}$  how

totally devastated.

(109) John's what I'd call peculiar.

Recall the behavior of true parentheticals. It appears that no quantity of word boundaries blocks these rules.

The parallel wanna examples unfortunately elude construction. Of course, if every to clause is a sentence, the wanna rule involves terms on either side of ## every time it applies. On the other hand, if every to clause is a sentence, the deleted (or unfilled) subject should block the rule.

Probably it is better (in general, not just for my purposes) to assume that to clauses are verb phrases, with interpreted-in subjects. Such an idea is not novel, occurring in various connections to Emonds (1970), Schachter (1976), Lasnik and Fiengo (1974), Brame (1976), Akmajian, Steele, and Wasow (1977), and Bresnan (1976). In Bresnan (to appear) is a really thorough working-out of the various justifications and consequences of this decision.

Another type of interesting phenomenon is represented in (110).

(110) John shouted out an "I know!"

"I know!" is surely a noun here. But is it also a sentence? That is, does the quotation have internal structure?

(111)  $[_N [_S [_S [_N [_{PRO} I ] ] ] [_V [_V [_V \text{ know} ] ] ] ] ] ]$

I believe that the answer is no, even though the opposite conclusion appears at first sight to be useful against boundaries, in the same way as (107). Nevertheless, saying that quotations have syntactic analyses on the same order as the ones we'd give normal sentences amounts to saying that a grammar is responsible for generating them, whereas, obviously, any kind of noise, grammatical or not, English or not, human or not, is possible in place of "I know!", which happens to be human, English, and grammatical. If a grammar generated quotations, it would be impossible to say

(112) "X" is ungrammatical.

where X is in fact ungrammatical.

Partee (1973), against this conclusion, points out that the contents of quotations are accessible to anaphora. An example:

(113) John said, "Italktoofast," thereby convincing me  
that he did.

I'm not sure what this shows. Note (114):

(114) John said, "Je parlons francais," thereby convincing me that he didn't.

Interpretation of the absent  $\bar{V}$  in (114) evidently requires



knowledge of matters outside the language, as it does in more famous (Hankamer and Sag 1976) examples like (115).

(115) Don't!

The interpretive processes which give sense to these kinds of sentences certainly do not require something labeled  $\bar{V}$  elsewhere in the sentence or even in the discourse, in particular not inside the quotes in (110).

The fact is that all true grammatical rules behave as if quotations had no internal structure.

(116)(a) John can say "who" in Gaa.

(b) \*"Who" can John say in Gaa?

(117)(a) It seems "who" is the only thing John can say  
in Gaa.

(b) "Who" seems to be the only thing John can say  
in Gaa.

In (116), "who" is not a WH-word, merely a noun phrase, and hence is off-limits to WH-movement. But rules which move noun phrases around, regardless of their contents, are quite capable (117) of moving quotations.

Note too examples like

(118) John shouted out an "I know! I know!"

Here we still want to say that the quotation is a noun. It begins in a vowel, so an appears for underlying a. Now

"I know!" and "I know!", if sentences, must be syntactically independent, as much as any pair of sentences in a discourse.

This appears to conflict with their both being dominated by N. Evidently we are forced to conclude that "I know! · I know!"

does not contain two adjacent sentences. Further, if "I know!" were a syntactically independent sentence, like a parenthetical, then a should regularly appear, as before a parenthetical. But in fact there is no distinction between (118) and (110). There is no vowel-initial quotation that will not get an. John could shout out the entire pentateuch without its blocking the a/an rule.

(119) John, highly agitated, leaped to his feet and blurted out an "In the beginning..."

Likewise, in (120),

(120) "A" occurs before consonants.

if "a" were analyzed as a determiner, we'd be forced to say

(121) "An" occurs before consonants.

which is undesirable, and also untrue. Note that, because of examples like (110), the theory that there is a "quotation boundary" blocking high-level rules fails. Rather, the contents of the quote are extralinguistic. I'll return to this matter in Part IV.

In the rest of this work I will consistently eschew the use of boundaries. (The same decision has recently been made, though for different sorts of reasons, by Selkirk (to appear).) Still, I will time and time again compare, with reference to specific problems, boundary solutions and no-boundary solutions. The running point of these comparisons will be that boundaries are incapable, by their very nature, of dealing properly with the real generalizations. But the theory that is capable of dealing with the real generaliza-

tions can also reproduce the past results of boundary theory, with, it turns out, a more accurate fit to the facts.

It has not escaped my notice that every single one of the rules discussed in this Part involves non-lexical categories. This has a natural explanation. I refer the reader to Part III below.

## APPENDIX TO PART I

Boundary Theory: History

I'd like to make a couple of remarks on the historical origins of boundary theory, as it is not immediately clear how linguists got into the habit of thinking of boundaries as palpable, manipulable juncture elements.

"Boundaries," in a sense that will become slightly clearer, were (first, to my knowledge, in Western traditions) recognized by Prague Circle linguists. The best disquisition on the subject is found in Trubetzkoy (1939). The observations in this book are not really oriented towards theory; they consist largely of a fairly harmless, though extensive, catalogue of boundary phenomena in phonology, classified in the usual ingenious fashion. "...jede Sprache...spezielle phonologische Mittel besitzt, die das Vorhandensein oder das Nichtvorhandensein einer Satz-, Wort-, oder Morphemgrenze an einem bestimmten Punkt des kontinuierlichen Schallstromes signalisiert..." These Grenzsignale include all phenomena accounted for by rules which, as rendered by post-SPE phonologists, have sentence, word, or morpheme boundaries in their structural conditions--all phenomena which, to Trubetzkoy's way of thinking, perceptually reveal the limits of these units. "Sie dürfen wohl mit den Verkehrssignalen in den Straßen verglichen werden." The functional explanation is every bit as bald as one begins to suspect: The reason

for the existence (e.g.) of the Greek spiritus asper or English ʔ, occurring only in word-initial and -final position respectively, is the demarcation of words. And "die kombinatorischen Varianten [allophones] sind nicht bloß kasuell bedingt Naturphänomene, sondern teleologisch bedingte Erscheinungen, die einen bestimmten Zweck haben und eine bestimmte Funktion ausüben. Diese Funktion besteht immer in der Signalisierung der unmittelbaren Nachbarschaft eines anderen Sprachelementes..." This we need not accept, and I will have more to say about it shortly.

(Trubetzkoy classifies Grenzsignale first as phonemische or aphonemische. The former are distinctive oppositions with restricted privilege of occurrence (i.e. initial or final only); these would now be handled by conditions on word or morpheme structure. With his customary unbelievable erudition, Trubetzkoy notes that "dies ist der Fall z. B. bei den aspirierten Verschlusslauten des schottischen (gaelischen) Dialektes der Insel Barra, bei den aspirierten und bei den rekursiven Konsonanten des Ostbengalischen, bei den rekursiven Verschlusslauten und emphatischen-mouillierten Konsonanten des Tschetschenischen..." The latter is edge-conditioned allophony. "Im Obersorbischen wird der stimmlose gutturale Reibelaut x im Morphem anlaut als gutturale Affrikata kx... gesprochen..." Trubetzkoy includes stress in this classification.

(Grenzsignale also fall into Einzelnsignale, like all the examples above, or Gruppensignale--special combinations of

elements occurring only at boundaries. The English clusters *θs*, *fz*, *sθ*, *zθ*, *čt*, *čs*, *sš*, etc. are examples of what Trubetzkoy has in mind. Finally, Grenzsignale may be positive, like all the examples already cited, or negative, meaning that they signal the absence of a boundary. "Im Efik kommen h und r nur im Inlaut vor..." Vowel harmony phenomena go here as well.

(These three distinctions cross-classify, and Trubetzkoy provides many examples. I go through this only to transmit a feel for the vast range of phenomena Trubetzkoy considers.)

Trubetzkoy's picture of boundaries is rather healthy, in my opinion. Since he has no notation, he is not tempted to use boundaries notationally, and it certainly does not occur to him to make phonemes out of them. A boundary, in his usage, is no more than the left or right edge of any meaningful element, not any sort of formal object of whose properties one may legitimately speak. I imagine it is similar to, say, the modern idea of discourse, which certainly exists as a kind of confluence of true formal objects, but which does not, at least not so obviously, have non-trivial characteristics of its own that do not follow from those of the smaller linguistic elements to which it owes its existence. This point of view I find myself in agreement with. Trubetzkoy is merely observing that linguistic phenomena (if not rules yet) are in some sense limited to explicit domains of influ-

ence, which he enumerates. (As we know, however, once this unexceptionable "boundary" gets itself symbolized by rule-writers, the temptation to regard it as a manipulable, operable-upon element like the others becomes irresistible.)

However, the idea that these phenomena are so limited merely in order to demarcate the speech signal is a little offensive, because it does not allow us to hope for interesting and (teleologically speaking) arbitrary properties of the rules behind them. On the subject of stress rules, for instance, Trubetzkoy remarks that they "dienen nur zur Signalisierung der Nähe der Wortgrenze..." He goes on: "So weit der 'gebundene Akzent' [predictable stress] eine Wortgrenze angibt, hat er eigentlich nur im Satzinnern einen Sinn. In einer Sprache, wo in jedem Worte die letzte Silbe betont und somit die Schlußgrenze des Wortes angegeben ist, sollte eigentlich diese Endbetonung beim letzten Worte eines Satzes unterlassen werden, da in diesem Falle die Schlußgrenze des Wortes ohnehin durch die Schlußpause des Satzes genügend signalisiert ist. Dies ist auch tatsächlich in vielen Sprachen der Fall. Nach E. D. Polivanov soll im Koreanischen jedes Wort die Schlußsilbe betonen, und nur das letzte Wort im Satze betont die Anfangssilbe..." Following this reasoning, stress rules should also fail in words spoken in isolation, in parenthesis, or with list intonation. Surely the facts he mentions are themselves higher-order boundary phenomena, to be accounted for by sentence-level rules.

I might also cite the obvious existence of example after example of bread-and-butter rules of external sandhi which conspire to destroy Grenzsignale. French liaison, for instance, which effectively inserts a word-final consonant intervocalically, obliterates VV sequences which, left unmolested, would beautifully mark word boundary. Quite routinely, in fact, deletion and insertion rules at word edge (there are examples in almost every Indo-European language) make at least a half-hearted attempt to generalize word-level phonotactics and syllable structure over the sentence. Still more striking is the fact that such effects, when related to speed of utterance, occur during rapid delivery, when decodability and segmentability are presumably at a premium. I really doubt that any broad functional explanation for the existence of boundary phenomena (let alone their properties) is going to get off the ground. I do not offer any kind of explanation myself, because in my opinion there isn't one, any more than there is an explanation for the existence of transformations. I can at least conceive of a phylogenetic explanation for the integration of both of these into the linguistic faculties, but it would surprise me to find anything to explain why they enter into contemporary, already-evolved linguistic systems. (I do have explanations for the curious facts about boundary effects which I mentioned; see Part IV.)

Trubetzkoy himself mentions that "in Bezug auf die Verwendung der Grenzsignale sind die einzelnen Sprachen sehr



verschieden. In den einen Sprache werden hauptsächlich (oder sogar ausschließlich) die Morphemgrenzen signalisiert, in den anderen die Wortgrenzen." This corresponds to the mundane observation that some languages have busier word-level phonologies, while others have busier morphologies. "Es gibt Sprachen, die nicht nur sehr wenig Grenzschnale besitzen, sondern sie auch sehr selten verwenden, so daß nur ein ganz unbedeutender Prozentsatz aller Wort- (bezw. Morphem-) grenzen in einem zusammenhangenden Texte 'signalisiert' sind. Zu solchen Sprachen gehört z. B. das Französische, das auf das Abgrenzen der Wörter (bezw. Morpheme) im Satze sehr wenig Wert legt." In these passages Trubetzkoy seems to be doing himself in. This kind of variability in elaboration of Grenzschnale evidently conflicts with his explanation for their existence, assuming (to adopt his idiom) that one language needs to delimit its units as much as another. In my opinion Trubetzkoy underestimates (actually, fails to consider) the contribution of the lexicon to segmentation.

Juncture symbols appeared in American structuralist work at about the same time as the Grundzüge (Sapir and Swadesh 1939, Trager and Bloch 1941). The functionalist slant is of course absent, but essentially similar phenomena are at issue: Asymmetric distribution and allophony respecting unit edge. The boundary symbol is just a way of notating the edge, which Trubetzkoy did not find it necessary to do. But by the time of Harris (1947) and Trager and Smith (1951) junctures had

taken on a really dangerous theoretical status. I will elucidate.

Harris sets up junctures whenever he wants to reduce phoneme inventories or to express parallel distributions of sets of phonemes. So, for instance, he observes (drawing from Trager and Bloch (1941)) the [ay] of minus and the [Ay] of slyness, "distinguished chiefly by length and type of off-glide," dislikes the idea of both /ay/ and /Ay/, and accordingly sets up a juncture /-/ which can then be said to condition the distinction: m/ay/nus, sl/ay-/ness. Investigating further, he notices pl[ey]ful, tr[Ey]full, etc., whose parallel distinction the same subterfuge will account for. Likewise wholly different alternations, e.g. the respective aspiration and nonrelease of the k in market and What a lark! So far, so good: /-/ is just a way of writing the morphological edge, although we are not allowed to admit that yet: "Our arrangement is useful because it will later appear that whenever the segment [k'] occurs there is a morphological boundary following it (a boundary which also occurs at utterance end), so that /-/ becomes a mark of that boundary."

In fact "the great importance of junctures lies in the fact that they can be so placed as to indicate various morphological boundaries." (Harris is using the word juncture to refer to his formal constructs, boundary to correspond roughly to Trubetzkoy's Grenze.) "For example, replacing Swahili VCV by VCV# is particularly useful because the V

following  $\acute{V}$  is regularly the end of an independent morphological element..." But "things do not always work out so nicely. In German, we find [t] but not [d] before # ([bunt] 'group', [vort] 'word'), while [t] and [d] occur in identical environments within utterances ([bunde] 'in group', [bunte] 'colored', [vorte] 'in word'). If we insert # after every [t] and then group [t] and [d] into one phoneme, we would find that we are writing # in the middle of morphemes (e.g. /d#ayl/ Teil 'part'). We could still phonemicize [t] as /d#/, i.e. use the /#/ to indicate that a preceding /d/ represents the segment [t], but many of the occurrences of this /#/ would not correlate with morphological boundaries."

In this passage, amazing to modern ears, we realize what lies within Harris' power. A juncture, according to him, is just a formal device to allow for simplification of phoneme inventories. It may or may not correspond to the position of any boundary, which at this stage of the analysis has not been discovered anyway. We might, in a whimsical frame of mind, start by eliminating English /t/ and replacing it with /d@/. "By the setting up of junctures, segments which had previously contrasted may now be associated together into one phoneme, since they are complementary in respect to the juncture." We can now write a rule taking /d/ to [t] before /@/. The entire voiceless obstruent series can be eliminated with the same juncture, which we might take as a point in its favor. Given enough juncture symbols, in fact, we could easily make every English phoneme a positional variant of /g/.

"The fact that one phonemic juncture has been recognized does not preclude the recognition of additional independent phonemic junctures."

The reason that there is nothing in particular to stop such a laughable proliferation of ad hoc junctures is that they are not identified with boundaries (in Harris' and Trubetzkoy's sense), i.e. edges, which are reassuringly exiguous. Harris' method (or the more widespread practice it makes explicit) further discourages any such identification because junctures have to be phonemes, while boundaries are surely not: The environment of a phoneme is defined as the phonemic elements on its left and right. Junctures are also phonemes in Trager and Smith (1951).

Putting the matter a little more strongly, it is not entirely unreasonable to blame the introduction of this type of string juncture symbol on the American structuralist method itself. Such symbols are necessary precisely because of the inflexibility of the bottom-up analytical procedure. At the level that they first appear, they are merely place-holders for the real "boundaries," which the linguist is betting will show up at the higher levels. But given the possibilities for overzealous reduction which I mentioned, the linguist will sometimes be disappointed.

(In a theory like mine, however, which has no juncture elements, only an all-purpose symbol meaning 'edge of domain,' these problems do not arise, because the edges are where they are, not where we feel like putting them. All "boundary phe-

nomena" are direct reflections of constituency at the various levels of structure. Only as many sets of boundary phenomena--domains of application--may exist as there are levels.)

In SPE theory, the next major development, junctures and boundaries (both in Harris' sense) are one and the same. A juncture is no longer merely a symbol but a terminal element of which one expects as much epistemological significance as one does of any other element. The rationalist underpinnings of the book force this identification. But now that boundaries have formal existence in mental representation, it seems reasonable to want to manipulate them like the others; hence the abuses I mentioned earlier. Boundaries are once again diacritics, but now diacritics with psychological reality.

The reason, incidentally, that boundary symbols are now used only in phonology is that they were inherited directly from American structuralism, which of course had not recognized the exactly parallel "boundary effects" in syntax.

## PART II

### THE IRISH INITIAL MUTATIONS

As an extended second example of high-level morphology and what I believe is generally true of it, I want to discuss the initial mutations of Modern Irish. What's meant by this term is a series of rather spectacular alternations undergone by the initial consonants of words, under certain interesting conditions. The rules responsible for the mutations have properties and limitations which are remarkably similar, at a certain depth of analysis, to those of the English rules of Part I.

Special thanks go to Ken Hale for suggestions and comments on the Irish.

The dialect I cite is "standard" Munster Irish throughout, with the occasional Galwayism, though all dialects are substantially the same with respect to the mutations. General information on the mutations is easily had; see, for instance, de Bhaldraithe (1953), Dillon and Ó Cróinín (1961), Ostedal (1962), Mhac an Fhailigh (1968). Initial mutations in some form are in fact pan-Celtic phenomena, showing up in all of the several languages of the Celtic group.

There are two major mutations, known among Celticists as lenition (sometimes "aspiration") and nasalization (sometimes "eclipsis"). By lenition, extremely roughly speaking, stops spirantize and spirants either turn to [h] or else delete.

By nasalization, unvoiced consonants voice and voiced ones nasalize. In the table below I've represented in detail the effects of the two rules.

			<u>lenited</u>		<u>nasalized</u>	
(1)	p	p', p	ph	f', f	bp	b', b
	t	t', t	th	h', h	dt	d', d
	c	k', k	ch	c, x	gc	g', g
	b	b', b	bh	v, w^	mb	m', m
	d	d', d	dh	j^, ʃ	nd	n', n
	g	g', g	gh	j^, ʃ	ng	ŋ', ŋ
	f	f', f	fh	zero	bhf	v, w^
	s	✓ s, s	sh	h', h		
	m	m', m	mh	v, w^		

Irish consonants, written in Irish as in the first column, come in (contrastive) palatalized and velarized varieties; these are represented phonetically in the second column. I use the apostrophe to indicate palatalization whenever the required distinctions are not made by normal IPA symbols, which is most of the time. In the third column are the written lenited consonants: Ch, C any written unlenited consonant. Next are the pronunciations of the lenited palatalized and lenited velarized consonants, respectively. In the fifth column are the written nasalized consonants: NC, where C is any written unnasalized consonant and N is the spelling of the result of nasalizing the consonant C represents. Last are the pronunciations of the nasalized palatalized and nasalized ve-

larized consonants. [j<sup>h</sup>] is a voiced palatal fricative; [w<sup>h</sup>] is a voiced bilabial fricative with lip rounding.

To a reasonable first approximation, the rules behind these alternations are (2) and (3). Though their phonological details are not my primary concern here, it will be nice to have concrete rules to refer to.

$$(2) \left[ \begin{array}{c} +\text{cons} \\ -\text{vce} \\ \text{✗cor} \\ +\text{cont} \end{array} \right] \rightarrow \left[ \begin{array}{c} +\text{cont} \\ -\text{son} \\ -\text{✗cons} \\ \langle \text{✗unit} \rangle \end{array} \right] / [+L] \_\_\_$$

$$(3) \left[ \begin{array}{c} +\text{obst} \\ \text{✗vce} \end{array} \right] \left[ \begin{array}{c} \text{✗nas} \\ +\text{vce} \end{array} \right] / [+N] \_\_\_$$

Notice that both rules have a distinctly morphological smack to them: Their applicability turns on the presence in the preceding word of an abstract morphological feature [+L] or [+N], whose sole function is to exact the proper mutation from the following initial. No word is both [+L] and [+N], although many are neither. I assume, then, that [+N] implies [-L], and [+L][-N], and also that all words which do not cause mutations are [uL, uN]. That is:

$$(4) [\text{✗L}] \equiv [-\text{✗N}]$$

$$[uL] \equiv [uN]$$

A reasonable idea at this point is to reduce these to a single binary-valued feature, [+IM], such that [+IM] is equivalent to [+L] and [-IM] is equivalent to [+N]. The implications corresponding to those in (4) now come automatically. We rewrite (2) and (3) as

$$(2)' \quad \dots / [+IM] \_\_\_$$

$$(3)' \quad \dots / [-IM] \_\_\_$$



These features, as we will see, are lexical properties of words, mostly escaping generalization other than historical. As further support for the morphological character of (2)' and (3)', I observe that there is a collection of more clearly morphological rules which must follow. (I am assuming, of course, some version of the autonomous morphology thesis: See Aronoff (1976), but Carrier (1978), Aronoff (1978).)

An example. The homophonous possessive pronouns a 'his', a 'their', and a 'her' differ only in the mutations they cause.

- (5)(a) a bhó 'his cow'
- (b) a mbó 'their cow'
- (c) a bó 'her cow'
- (d) bó 'a cow'

A 'his' is marked [ $\neq$ IM], taking the following [b] to [w<sup>h</sup>]. A 'their' is [-IM], taking the same [b] to [m]. A 'her' has no marking, and induces no mutation.

Now the preposition ag 'at, by, of' together with a following a (any of the three) surface as a lexically suppletive form á. (The phonology which might produce this would have no generality whatsoever.) The rule is probably a "stupid" one, similar, say, to the rules taking à le to au, in dem to im, and want to to wanna--saying no more than (6):

- (6) ag a  $\rightarrow$  á

But á still behaves like three words with respect to the mutations:

- (7)(a) á bhó 'his cow's'  
 (b) á mbó 'their cow's'  
 (c) á bó 'her cow's'

The point is that, to avoid multiplying á's, the mutations must precede the disappearance of the three a's via (6).

Similarly, the possessive pronoun mo [mʌ] 'my' induces lenition.

- (8) mo bhó 'my cow'

It also loses its carat before a noun beginning in a vowel.

- (9) m'ainm 'my name'

This elision, like similar rules in a great many languages, is limited to a small set of specifiers, and is not phonological in nature. What happens, we might ask, before f?

- (10)(a) \*mo fhear [mʌ x:r]  
 (b) m'fhear [m x:r] 'my husband'  
 (c) fear [f x:r] 'a man'

The f lenites--deletes--by (2)', then elision.

It is reasonably easy to conclude from things like this that the mutations are not phonological rules in the same spirit as, for instance, French liaison or Italian raddoppiamento, for which see Part III.

Both lenition and nasalization had real phonological conditioning in Common Celtic, the ancestor of the modern Celtic languages. Lenition originally occurred after word-final vowels, nasalization after word-final nasals. But the idea of reconstructing these segments in the lexical representations of the modern Irish words is a bad one. First, the attrition

of Common Celtic endings was rather brutal, often wiping out syllables or more. Therefore, for example, proposing a phonotactically reasonable underlying final -N/ for a word in -CJ which causes nasalization will entail, in most cases, supplying an entire underlying final syllable -VN/ . It would have to delete unconditionally: There are, to my knowledge, no phonological reflexes of the lost endings apart from the mutations. A further misfortune for this line of thought is the fact that modern Irish words which actually do end in vowels and nasals do not reliably cause anything in particular. Supposing we wanted to make the mutations phonological rules, we would be forced to protect these with underlying finals which also wound up deleting by special rules. This seems moderately artificial. Even on a priori grounds, we can be sure it's wrong. (These remarks are by way of defense of my abstract treatment of the mutations, which, although tolerably natural and easily the least involved, seems on principle to make certain phonologists nervous.)

The collection of phenomena generally referred to as the mutations falls into two classes, only one of which I am really concerned with here. The distinction between the two, not even recognized, to my knowledge, by any writer on the subject, is one I want to clearly draw. In the first, interesting class, a mutation is clearly induced in some way by the word to the left of the affected one. (I will often speak of words as causing and undergoing the mutations.) Mo induces lenition: This is indisputable, though so far without much

theoretical content. Words after mo always lenite, and the same words unpreceded by mo (or any other known lenition-inducer) do not.

In the other class, it is not in my opinion profitable to think of the mutation as having anything at all to do with the preceding word. Rather, it is part and parcel of the number-, gender-, case-, definiteness-, and tense-marking systems. Nouns, for instance, when genitive, masculine, singular, and definite, show up with lenited initials.

(11)(a) capall Shéamais

horse Séamas-GEN-M-SG-DEF

'Seamas' horse'

(b) mac Sheáin

son Seán-GEN-M-SG-DEF

'Sean's son'

(c) póca chóta na mná

pocket coat-GEN-M-SG-DEF the woman-GEN

'the pocket of the woman's coat'

It would be a mistake to imagine that the nouns capall, mac, póca are in any sense inducing lenition, because the same effect occurs when an article precedes the genitive noun.

(12)(a) capall an fhir

horse the man-GEN-M-SG-DEF

(b) mac an bháicéara

son the baker-GEN-M-SG-DEF

(c) póca an chóta

pocket the coat-GEN-M-SG-DEF

Further, if we merely substitute an adjective for the possessor noun, it remains intact:

- (13)(a) capall mór  
horse big
- (b) mac díomhaoín  
son unemployed
- (c) póca beag  
pocket small

Yet a following possessor still lenites:

- (14)(a) capall mór Shéamais
- (b) mac díomhaoín Sheáin
- (c) póca beag chóta na mná

Notice too that if the head noun is itself genitive, masculine, singular, and definite, the adjective lenites.

- (15)(a) ceann an chapail mhóir  
head the horse-GEN-M-SG-DEF big-GEN-M-SG-DEF
- (b) deartháir an mhic dhíomhaoín  
brother the son-GEN-M-SG-DEF unemployed-GEN-M-SG-DEF
- (c) tóin an phóca bheag  
bottom the pocket-GEN-M-SG-DEF small-GEN-M-SG-DEF

The proper conclusion is surely that the allomorphy triggered by the grammatical marking of the noun ((11), (12), (14), (15)) or the adjective ((15)) includes mutation of its initial, just as it (often) does vowel ablaut and change in quality of final consonant.

(16)(a) an mac [mak]

the son

(b) an mhic [vik']

the son-GEN

The preceding word is irrelevant. To suggest, as investigators usually do, that lenition occurs "after" a possessee noun, or "after" a genitive article, and so on, is in my opinion missing the point of the thing. This point is that the bundle of features [+GEN, +M, +SG, +DEF] is morphologically signaled by a lenited initial.

This kind of treatment explains facts like the following. Adjectives also lenite when qualifying a noun which is feminine, nominative, and singular, i.e. when marked that way themselves. This marking must have occurred, because adjective and noun agree morphologically in respects other than lenition.

(17)(a) an bhean mhór

the woman-NOM-F-SG big-NOM-F-SG

(b) na mná móire

the woman-GEN-F-SG big-GEN-F-SG

(c) na mná móra

the women-NOM-F-PL big-NOM-F-PL

(d) na mban mór

the women-GEN-F-PL big-GEN-F-PL

Multiple adjectives also lenite, providing a few difficulties for a theory trying to make the immediately preceding noun responsible, i.e. [+IM].

(18) an bhean mhór shaibhir

rich

But a demonstrative, presumably not marked for gender, number or case since not visibly inflected for them, does not lenite.

(19) an bhean { sin }  
                  { \*shin }

that

'that woman'

Similarly, a predicate adjective is evidently inaccessible to grammatical marking, taking the unmarked NOM-M-SG form regardless of the preceding noun phrase.

(20) Tá { an bhean }  
          { an fear }  
          { na mná } cliste.  
          { na fir }

be { the woman }  
      { the man } smart  
      { the women }  
      { the men }

The prediction is that lenition will also fail. It does. Compare:

(21) Tá an bhean cliste.

is the woman smart

'The woman is smart.'

(22) Tá an bhean chliste saibhir.

rich

'The smart woman is rich.'

(23) Tá an bhean chliste shaibhir brónach.

unhappy

'The smart rich woman is unhappy.'

(24) Tá an bhean chliste shaibhir bhrónach anseo.

here

'The smart rich unhappy woman is here.'

Similarly, instead of saying (Dillon and Ó Cróinín 1961, de Bhaldraithe 1953) that lenition occurs "after" a noun in the dual number,

(25) dhá mhuic mhéithe

two pigs fat

it is less misleading to say that the bundle of features [-SG, -PL], dual, gets inflectionally realized as a lenited initial. Notice, incidentally, that the dual noun itself, mhuic, suffers precisely the same mutation. This, of course, is what we expect. Usually, however, it is accounted for separately, by saying that lenition occurs "after dhá," without which article the dual is not found.

(26) \*mhuic mhéithe

\*muic mhéithe

'fat pigs-DUAL'

As a final objection to the idea, I think probably based on historical considerations, that a mutation must be induced by some other word, I observe that verbs, unpreceded by anything in the normal VSO sentence, lenite spontaneously, without benefit of any possible trigger to the left, as part of the morphological marking of the preterite, the past habitual,



and the conditional. The bare verb stem, for instance, visible in the imperative, lenites to form the "personal" preterite.

(27) B<sup>í</sup> ag dul abhaile.

be at going home

'Go home.'

(28) Bh<sup>í</sup> sí ag dul abhaile.

be-PST she

'She was going home.'

(29) Cuir an leabhar anseo.

put the book here

'Put the book here.'

(30) Chuir sí an leabhar anseo.

put-PST

'She put the book here.'

Optionally present in some dialects is the verbal prefix do-, also marking the preterite: do-chuir, etc. Do- is entirely absent in many areas. It is important to show that, even though historically responsible for the now morphologized lenition, do- is not [+IM]. Note first that lenition in the preterite is areally invariant and obligatory, though do- actually occurs with great inconsistency. Second, Ken Hale points out to me that the (optional) do- now also occurs with the "impersonal" (essentially passive) preterite, where lenition does not. So, for instance,

(31)(a) \*(Do-)cuir sí an leabhar anseo.

(b) (Do-)chuir sí an leabhar anseo.

(32)(a) (Do-)cuireadh an leabhar anseo.

(b) \*(Do-)chuireadh an leabhar anseo.

'"They" put the book here,' i.e. 'The book got  
put here.'

In the future, we find the stem adorned with the future ending.

(33) Cuirfidh sí an leabhar anseo.

put-FUT

'She'll put the book here.'

Compare the habitual and conditional, which, in addition to taking special endings, also lenite.

(34) Chuireadh sí an leabhar anseo.

put-PST-HAB

'She used to put the book here.'

(35) Chuirfeadh sí an leabhar anseo.

put-COND

'She'd put the book here.'

This kind of example appears to present terminal difficulties for the theory that something must "cause" mutation across w-juncture. Rather, somewhat like strong verb ablaut in English or umlaut in German (marking tense and number respectively), the once contextually-conditioned alternations, detached from all segmental provocation by reason of its historical loss, have been reanalyzed as productive inflectional processes.

There are a good many examples of the preceding type in Irish, involving clusters of grammatical features triggering

one mutation or the other. I will not enumerate them here (they are slightly beside the point), but content myself with noting their existence. Nevertheless, we surely want (2)' and (3)' to be responsible at some level, because the alternations are in every respect identical to those induced by words of the mo sort. I now turn to this problem.

Remaining is a very large residue of mutations not traceable to the inflectional makeup of the word containing the mutating segment. Mo, for instance, cooccurs with lenition of the following word absolutely regardless of any markings it, the following word, may have received.

(36) fear M 'a man'

bó F 'a cow'

		M	F
NOM	SG	m'fhear	mo bhó
	PL	m'fhir	mo bha
GEN	SG	m'fhir	mo bhó
	PL	m'fhear	mo bhó

It strikes me as a really remarkable fact that every single one of the words which we must conclude are inducing mutations across w-juncture is non-lexical. The possessive pronouns, for instance, almost all induce one mutation or the other.

(37)		SG		PL
1	mo	[+IM]	ár	[-IM]
2	do	[+IM]	bhur	[-IM]
3	F a		a	[-IM]
	M a	[+IM]		

mo chapall	ár gcapall
do chapall	bhur gcapall
a capall	a gcapall
a chapall	

'my horse'	'our horse'
'your-SG horse'	'your-PL horse'
'her horse'	'their horse'
'his horse'	

Clearly a small subgeneralization is available in this instance.

$$(38) \begin{bmatrix} \text{A PL} \\ \text{uF} \\ \text{mIM} \end{bmatrix} \rightarrow [-\text{A IM}]$$

Among non-lexical items capable of bearing these features, i.e. inflected nominal specifiers, plurality entails nasalization and singularity lenition. A 'her', alone marked for femininity, causes no mutation. We could, of course, simplify (38) a bit by making a [uIM]; the other possessives, in any case, are simply [mIM].

The quantifier aon 'one' and the associated ordinal céad 'first' both lenite:

(39) aon chapall

(40) an chéad chapall

Seacht, acht, naoi, and deich, 'seven', 'eight', 'nine', 'ten' all nasalize the next word:

(41) seacht gcapaill

(42) acht gcapaill

(43) naoi gcapaill

(44) deich gcapaill

Notice the plural gcapaill. Now trí, cheithre, cúig, sé, 'three', 'four', 'five', 'six' all lenite.

(45) trí chapall

(46) cheithre chapall

(47) cúig chapall

(48) sé chapall

Notice the singular chapall. The inescapable conclusion is that aon, trí, cheithre, cúig, and sé are [mIM, -PL], hence, by (38), redundantly [+IM]; seacht, acht, naoi, and deich are [mIM, +PL] and thus [-IM].

Other numerals, e.g. fiche, tríocha, daichead, 'twenty', 'thirty', 'forty', cause no mutation.

The nominative singular definite article, an, lenites feminine nouns, but does nothing to masculine ones.

(49) an bhó

(50) an fear

It appears we must list both an [+NOM, +F, +SG, +IM] and an [+NOM, +M, +SG, uIM]. Similarly, the dative singular article, yet again an, is [+DAT, +SG, -IM]:

(51)(a) capall 'a horse'

(b) bean 'a woman'

(52)(a) an capall 'the horse-NOM'

(b) an bhean 'the woman-NOM'

(53)(a) ag an gcapall 'at the horse-DAT'

(b) leis an mbean 'with the woman-DAT'

Finally, the genitive plural article, presumably [mIM] and

subject to (38), also nasalizes:

- (54) (a) cosa na gcapall 'the legs of the horses-GEN-PL'  
(b) deirfiúracha na mban 'the sisters of the women-GEN-PL'

Note that, unlike the situation in examples such as (18) and (24), in which mutation-by-grammatical-marking affects a noun and all like-inflected hangers-on, here we do not get nasalization of anything more than the noun directly to the right of na.

- (55) (a) cosa na gcapall { \*mbán }  
          { bán }
- white
- 'the legs of the white horses'
- (b) deirfiúracha na mban { \*gcóir }  
                                { cóir }
- just
- 'the sisters of the just women'

The degree modifiers an 'very' and ró 'too', occurring pre-adjectivally, are [+IM].

- (56) (a) maith 'good'  
(b) beag 'small'
- (57) (a) an-mhaith 'very good'  
(b) an-bheag 'very small'
- (58) (a) ró-mhaith 'too good'  
(b) ró-bheag 'too small'

Quite a few prepositions also induce lenition (assuming no covert cases in Irish), e.g. ar 'on', do 'to', ó 'from', trí 'through', gan 'without', fá 'under'.

- (59) ar bhó 'on a cow'
- (60) do Sheán 'to Sean'
- (61) ó Cheimbridge 'from Cambridge'
- (62) trí chorrach 'through a marsh'
- (63) gan bhó 'without a cow'
- (64) fá chloich 'under a stone'

The one preposition which nasalizes is i 'in'.

- (65) i mBoston 'in Boston'

Many miscellaneous verbal particles induce a mutation to their right. Pre-verbal ní 'NEG', má 'if', and the complementizer a lenite:

- (66) Creideann sé é.

believe he it

'He believes it.'

- (67) Ní chreideann sé é.

NEG

'He doesn't believe it.'

- (68) Má chreideann sé é, tá sé ar buile.

if is he on madness

'If he believes it, he's crazy.'

- (69) Is bréag mheabhlach an scéal a chreideann sé.

is lie malicious the story that

'The story that he believes is a malicious fabrication.'

Nasalizing are (e.g.) pre-verbal an 'Q', dá 'if', mara 'unless', and the complementizer go:

- (70) An gcreideann sé é?

Q

'Does he believe it?'

(71) Dá gcreideadh sé é!

if believe-PST-SUBJUNC

'If only he believed it!'

(72) Mara gcreideann sé é, tá sé cliste.

unless is he smart

'He's smart if he doesn't believe it.'

(73) Is iad na daoine go gcreideann sé a scéal.

is they the people that their story

'They're the ones whose story he believes.'

The copula induces lenition to its right in the past and conditional, which are homophonous in this case.

(74) Ba dhochtúir Máire.

is-PST/COND doctor

'Maire was/would be a doctor.'

Because just about anything can occur to the right of the copula, as opposed to most of the specifiers in the foregoing examples, the leniting copula is a particularly good illustration of the difference between lenition-by-sentence-morphology and lenition-by-grammatical-marking. We can assume, for instance, that case-marking affects only [+N] items, i.e. the nouns and adjectives. There is no case paradigm for, say, the possessive pronouns (37). (Recall (36).) This explains why we don't get things like

(75) \*capall mh'fhir

horse my man-GEN



'my husband's horse'

(where mo would have to be marked [+GEN, +M, +SG, +DEF], like fir), but rather

(76) capall m'fhir

Non-inflected items never mutate, when the mutation is associated with grammatical marking. But when it isn't,

(77) Ba shin é.

that it

'That was/would be it'

mutation regularly occurs. (Compare (77) with (19) above.)

Drawing all this together, lenition and nasalization are triggered by abstract features [+IM], [-IM], originating either (a) in the preceding word or (b) in the mutating word itself.

When and only when the feature comes from the preceding word, that word is non-lexical.

We must fix up (2)' and (3)' a little, since they account only for case (a); yet precisely the same alternations (1), I repeat, are characteristic of case (b). A reasonable emendation, it seems to me, is as in (2)", (3)",

(2)" .../  $\emptyset$   $\left[ \begin{array}{c} \text{---} \\ +\text{IM} \end{array} \right]$

(3)" .../  $\emptyset$   $\left[ \begin{array}{c} \text{---} \\ -\text{IM} \end{array} \right]$

supplemented by (78):

(78)  $[\alpha\text{IM}] [\text{uIM}] \rightarrow [\text{uIM}] [\alpha\text{IM}]$

(2)" and (3)" mutate only a word bearing the proper feature.

(I assume for now a normal feature percolation convention, whereby every segment of a word marked [+F] is also marked [+F].) This feature may arise as a byproduct of number, gender, case, definiteness, and tense marking. A series of redundancy rules marks words carrying certain bundles of features as either [+IM] or [-IM]. For instance, as we saw above ((25)),

(79)  $\begin{bmatrix} -SG \\ -PL \end{bmatrix} \rightarrow [+IM]$

dual marking entails lenition.

(78) transfers a mutation feature from the preceding word, provided it has one. It, the word on the left, must lose its feature in the process: go creideann, for example, becomes go gcreideann, not \*ngo gcreideann. I am assuming, of course, that (78) precedes (2)" and (3)". As for the ordering of (78) with respect to redundancy rules like (79), we have two choices. The fact is that (78) applies only to markings [+IM] which do not arise by grammatical rules like (79), i.e. only to those features which are associated from the start with particular non-lexical items. Otherwise, we would get

(80)(a) \*an bean shin

(b) an bhean sin

the woman that

'that woman'

(81)(a) \*Cuir shí an leabhar anseo.

(b) Chuir sí an leabhar anseo.

put she the book here

'She put the book here.'

(82)(a) \*Tá an capall Séamais mhór

(b) Tá an chapall Shéamais mór

is the horse Seamas-GEN big

'Seamas' horse is big.'

and the like. We could order (78) before (79) (etc.). However, this seems slightly suspicious to me, since I'd like to see grammatical marking preceding morphological rules like (78). The alternative is to restrict (78) to non-lexical items, which are exactly the ones which have [ $\pm$ IM] not from (79).

(78)'  $\left[ \begin{array}{c} \kappa\text{IM} \\ -\text{N} \\ -\text{V} \end{array} \right] [\text{uIM}] \rightarrow [\text{uIM}] [\kappa\text{IM}]$

I'll accept this for the time being, though it turns out below that we can simplify things a little. Right now, though, I'd like to point out that a boundary-strength account of the same facts is going to be inadequate. Suppose we recast (78)' as (83):

(83)  $[\kappa\text{IM}] \# [\text{uIM}] \rightarrow [\text{uIM}] \# [\kappa\text{IM}]$

This rule requires that [ $\pm$ IM] travel across a single word boundary. But this is not strong enough, as \*an bean # shin and \*cuir # shí point up.

Now (78)' is a high-level morphological rule. It applies across w-juncture. By its very nature, it applies only across w-juncture: We have no reason yet to imagine that the structural description of (78)' could ever be met word-inter-

nally, i.e. that the feature [ $\uparrow$ IM] is less than word-sized.

See, however, below.

By hypothesis, (78)' ought to behave like other rules of high-level morphology, for instance the ones discussed in Part I, in the characteristic respects. In fact it is true that the mutations apply across pause.

(84) Ea...dhochtúir í.

was doctor her

'She was...a doctor.'

Parentheticals, unfortunately, are impossible after non-lexical items, including all mutation-inducers.

(85)(a) \*Ba, deir sé, dochtuir í.

(b) \*Ba, dheir sé, dochtuir í.

(c) \*Ba, deir sé, dhochtuir í.

says he

'She was, he says, a doctor.'

On the other hand, a parenthetical is (slightly marginally) possible after the head of a genitive construction. Since, according to the theory, the mutation of the possessor noun is due to a word-internal feature, not one which travels across w-juncture by (78)', the preceding parenthetical should not affect it. This is true. To the extent that the parenthetical is syntactically natural, the genitive morphology is absolutely obligatory.

(86) Shílim gurbh é col ceathar, nó

I-thought that-be-PST him first cousin or

b'fheidir gaol eigin eile, Sheáin  
 maybe relative some other Sean-GEN

Uí Ríogáin.

O Reagan-GEN

'I thought he was a first cousin, or maybe some  
 other relative, of Sean O Reagan.'

As for trace, examples in the relevant places are a little hard to come by. I do have a couple of good ones, however. Recall that the pre-verbal particle ní 'NEG' lenites:

(87) Déanann sí é sin.

does she it that

'She does that.'

(88) Ní dhéanann sí é sin.

'She doesn't do that.'

When ni precedes the copula, however, the copula deletes.

(89) Is dochtúir í.

'She's a doctor.'

(90)(a) \*Ní is dochtuir í.

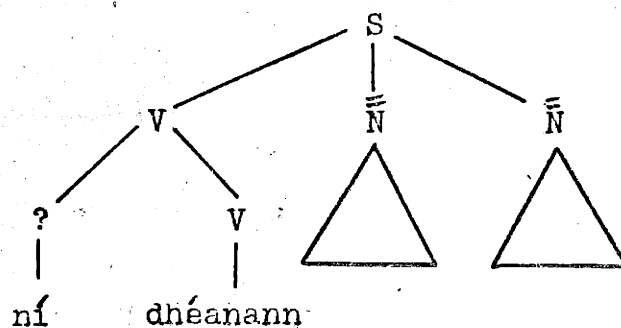
(b) \*Ní dhochtuir í.

(c) Ní dochtuir í.

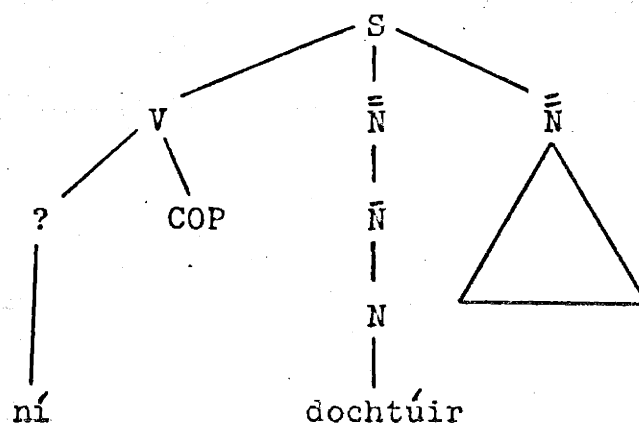
'She's not a doctor.'

Lenition is now impossible. Presumably, then, is left its pre-terminal structure behind when it went:

(88)'



(90)'



Now ní and dochtúir abut no one w-juncture, (78)' fails, and dochtúir is saved from lenition, as desired.

Note that a trace-as-word-boundary theory gives the wrong results here. The prediction of such a theory is that only removal of words belonging to lexical categories will end up producing extraction-site effects, since only they will have # on either side to leave behind. But here the deleted item is the copula, which is not lexical.

Non-lexicalness could, in principle, be something of a wild card. In this case, however, we have independent reasons not to think of the copula as a [+V] verb: Unlike a verb, it is impossible alone, stressless ("proclitic"), and in many circumstances deletable altogether. The very fact

that ba induces a mutation makes us want to class it with the unquestionably non-lexical items, which often do, rather than with the verbs, which never do.

The behavior of ní is no fluke. The interrogative an, which nasalizes, affords parallel examples.

(91) An bhfeiceann tu é sin?

Q see you it that

'Do you see that?'

The copula also deletes after an, as after all other pre-verbal particles:

(92)(a) \*An is feirmeoir í?

(b) \*An bhfeirmeoir í?

(c) An feirmeoir í?

'Is she a farmer?'

But now an no longer nasalizes.

There is a small list of such particles. The negative interrogative nach, to take a final example, also belongs on it. Nach nasalizes.

(93) Nach mbeannaigh sé thú?

NEG-Q salute he you

'Doesn't he salute you?'

(94)(a) \*Nach is báicéir í?

(b) \*Nach mbáicéir í?

(c) Nach báicéir í?

'Isn't she a baker?'

We must make sure, incidentally, that when (78)' fails in cases like (94), the [t̪IM] word loses its feature anyway.

The reasoning is slightly involved. Note that neither ní, an, nor nach could possibly be affected by either of the mutations I've mentioned. However, there is one I haven't mentioned, also set off by the feature  $[-IM]$ : Before vowels, n appears.

$$(95) \quad \emptyset \rightarrow n / \text{---} \left[ \begin{smallmatrix} V \\ -IM \end{smallmatrix} \right]$$

E.g.:

- (96) Nach n-ólann tú portar?  
 NEG-Q drink you porter  
 'Don't you drink porter?'

Note also:

- (97) (a) \*Nach (n-)is Albanach é?  
 (b) \*Nach n-Albanach é?  
 (c) Nach Albanach é?  
 'Isn't he a Scotsman?'

But now recall (92). When (78)' fails across trace, we do not end up with

$$(98) \quad *N\text{-}an \text{ feirmeoir } í?$$

which is what (95) would give unless we do something. We could prop up (78)' with a fairly trivial parenthesization:

$$(78)'' \quad \left[ \begin{smallmatrix} \kappa IM \\ -N \\ -V \end{smallmatrix} \right] ([uIM]) \rightarrow [uIM] [\kappa IM]$$

However, I prefer to think of facts like (98) as evidence that the feature  $[+IM]$  is not in fact word-sized, but rather segment-sized, occurring at word edge. An, for instance, is listed as (99), not (100).

$$(99) \quad [an[-IM]]$$



(100)  $\left[ \begin{smallmatrix} \text{an} \\ [-IM] \end{smallmatrix} \right]$

(2)" and (3)" become (2)''' and (3)''', and (95) becomes (95)':

(2)''' ... /  $[+IM]$  \_\_\_\_  
 (3)''' ... /  $[-IM]$  \_\_\_\_  
 (95)' ... /  $[-IM]$  \_\_\_\_ V

Notice now that (78)' is completely superfluous, on the assumption that the rules spelling out grammatical marking place  $[+IM]$  at left word edge. By giving  $[+IM]$  stringwise position, we avoid (98) into the bargain. The behavior of (2)''', (3)''', and (95)' across w-juncture (i.e. when  $[+IM]$  is not placed by grammatical spell-out) still follows from the same theory which predicted the behavior of (78)'. The fact that these rules do not breach w-juncture if the word on the left is lexical we can now account for by equipping only the non-lexical items as in (99). Lexical items never have mutation features at their right edges; thus we do not get (80), (81), (82).

The situation, then, is as follows. There are two possible positions of mutation-inducing features with respect to the initials they mutate: They end the preceding word, or else they begin the same word.

(101)(a)  $[+IM]] [ \text{ } ]$

(b)  $[ [+IM] \text{ } ]$

Both of these cases are covered by (2)''', (3)''', and (95)', which do not even recognize the distinction. However, just in case (a), where the terms of the structural description

turn out to correspond to factors on either side of a w-juncture, the predictable juncture effects show up.

Now I'm suggesting that it's inappropriate to write these rules with parenthesized boundaries. As it developed, writing

(102) ... / [~~t~~IM] (#) \_\_\_\_

is actually wrong. But suppose it were right. Even then it would be undesirable, because the notation implies that parenthesizing some other configuration (say the strongest occurring series) of boundaries would yield a possible output--one in which the juncture effects are not in evidence. It also implies that (2)''', (3)''', and (95)' could differ from one another with respect to their associated juncture effects. These, I believe, are not the right implications.

I'm going to conclude, then, that the mutations are respectful of w-juncture in the same way as the English rules of the first Part. Also in the same way, they are triggered only by non-lexical items. In the next Part I'll have a little more to say about this last matter.

## PART III

## SYNTACTIC CONDITIONING OF PHONOLOGICAL RULES

The English and Irish rules just discussed belong to a much larger class of phonological rules that allow a bundle of features on one side of a w-juncture to influence another on the other. We may consider, assuming that there are no boundaries, that these feature bundles are adjacent in a quite literal sense, and that therefore we're talking about a special class of local processes. Now a given rule of this class is either set off by some particular non-lexical item or set of items, as in the examples of Parts I and II, or else it isn't, and the rule is lexically productive--also a quite widespread phenomenon.

Most often a rule of this last sort will not produce sandhi between just any two words, supposing the phonological conditions on its application to be right. This selective failure is the problem to which I'm going to devote this Part. By way of explaining it, I cannot make the reasonable and somewhat standard assumption that some aspect of the intervening juncture, expressed in the quality or quantity of the terminal symbols separating the features involved, must have something to do with the further conditions that we'll evidently have to lay on the rule. Even so, I will constantly be pointing out in what follows that such an assumption is by its very nature incapable of dealing properly with the

facts.

Let me make parenthetically a quick and somewhat trite remark on the idea of terminality. Any feature of pre-terminal syntactic structure can in principle be persuaded to induce a special symbol @ in the terminal string, given appropriate procedures for the positioning and interpretation of @. We might contemplate doing this if, for example, we made up our minds that this aspect of pre-terminal structural information actually had to be made available to phonological rules, and if we held that phonological rules referred only to terminal symbols. Still, this kind of expedient would not be especially believable unless it could be demonstrated that @ had independent business in the terminal string, as does, for instance, #, in standard phonological theory. Selkirk (1972, 1974) wants to do precisely this for French, where, in her account (below), sandhi fails only across double word boundary.

In spite of what we might think of @, the reality of the information it carries, however graphically represented, is not really in question. We find such a symbol vaguely offensive only because it is redundant and because it blurs the distinction between phonological and structural information, otherwise nicely portrayed as terminal and pre-terminal information. This distinction is the one actually at issue. Is it in fact the truth that phonological rules have no purely syntactic conditioning?

I want to say here, to begin with, that some rules of

"selective" external sandhi ignore all independently apparent terminal properties of the junctures they breach--i.e. the properties traditionally represented by phonological boundaries--in favor of certain pre-terminal ones. I exclude the trivial sense in which @ is terminal; what I am really saying is that sandhi is not always conditioned simply by phonological features, and I want to present a theory of how I find it is conditioned. The notion w-juncture once more turns out to be crucially apropos.

In a recent series of studies (Rotenberg 1975, 1976, Kaisse 1977, Napoli and Nespor 1977, Clements, to appear), the point has been repeatedly made that SPE-style word boundaries can be as immaterial to phonological rules operating at gross phrasal levels as morphological boundaries can be to rules operating at the level of the word, or as both can be to rules operating at the level of the syllable (see Kahn 1976). There is a very real sense in which, for example, a pair of segments related by a phonological rule are adjacent even though a morpheme edge may happen to intervene. This in itself makes us begin to wonder in what sense a + boundary representing it actually exists. (The accepted convention for this sort of case is, of course to forgo writing + inside a pair of parentheses.) In a theory without #, the same goes for segments at word edge. Nevertheless, we must at some point correctly delineate the conditions that hold of rules relating them. The question of course arises, what do these conditions look like, anyway?

Surely we would like to say something interesting and falsifiable about exactly what sorts of information phonological rules are privy to.

In a very broad way, the key idea involved in such conditions is constituency. I'll show below that precisely the same idea is useful in a lot of other places in the grammar. Let us say, quite generally, that

(1) in a labeled bracketing  $\mathcal{A}$ , a sequence  $A$  of elements

$a_1 a_2 \dots a_n$  such that

(i) every  $a_i$  is a (prime member of the same natural class  $\underline{a}$  of units (these units specified by universal grammar and presumably including the lexical and prosodic ones of Part I), and

(ii)  $d_a(\mathcal{A}) = A$ , where

(a)  $d_a(\gamma) = d_a(\psi) d_a(\varphi)$  if  $\gamma = \psi\varphi$ ,  $\psi$  and  $\varphi$  non-null,

(b)  $d_a(\gamma) = a_k$  if  $\gamma = a_k$ , for any  $k$ , and

(c)  $d_a(\gamma) = e$  (the null string) otherwise

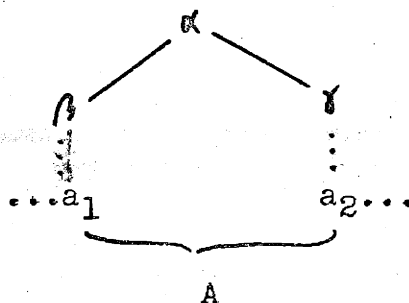
is a constituent  $\alpha$  if and only if there is a well-formed labeled bracketing  $\omega = [\alpha A]$ .

We'll say that  $A$  is a  $\alpha$ , and write  $(\alpha A)$ .

The idea of (1) is not difficult, more or less the standard is a. It allows us to say that a sequence  $A$  of elements  $a_1 a_2 \dots a_n$  is a constituent purely by virtue of its being ultimately and exhaustively dominated by a node  $\alpha$ . We find every  $a_i$  under  $\alpha$ , ignore everything else, and can now speak of each  $a_i$  and  $a_{i+1}$  as being both adjacent and co-constituent.

Say, for instance, we had a structure as in (2).

(2)



Then  $\mathcal{A} = [\beta \dots a_1 \dots] [\gamma \dots a_2 \dots]$

$d_a(\mathcal{A}) = A = a_1 \quad a_2$

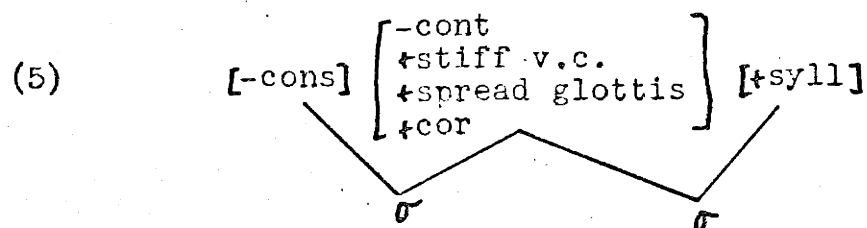
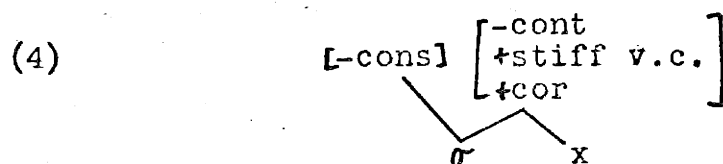
$\omega = [\alpha A] = [\alpha \quad a_1 \quad a_2]$

and we write  $(\alpha a_1 a_2)$ .

(1) is probably quite a handy notion. The important question being begged by it is the question, what sorts of labeled bracketings are there? Equivalently, what sorts of nodes  $\alpha$  are there? I imagine that the constituency predicate given in (1) will have to be employed in some form by phonological rules operating over any domain of linguistic structure where  $\alpha$  is defined, where there are in fact nodes.

Note that the theory of levels (Part I) becomes a special case of (1), where  $\alpha$  is from the set of levels  $\underline{L}$ . That is, (1) effectively allows us to say that a rule  $R$  is an  $\alpha$ -level rule, where  $\alpha$  is any node, not just one at the root of a tree at some level of representation. At most levels of representation, however, there is no distinction, since the trees are fairly flat. The node at the root of the tree is labeled with the same label as any other nodes that there might be. Phrase structure, of course, is the notable exception, and it is in the case of phrase structure that (1) is especially useful.

A first example. In the work of Kahn (1976), we can see that this generalized notion of constituency is precisely the relevant primitive. Kahn offers a powerful demonstration that syllable structure directly conditions rules of phonology. In his account of the allophones of American English /t/, for instance, Kahn shows that voiceless stops (including /t/) are aspirated when syllable-initial and non-syllable-final; that /t/ is glottalized when non-syllable-initial and directly following a non-consonant; and that /t/ is flapped when ambisyllabic, i.e. dominated by two adjacent nodes  $\sigma$ . In Kahn's notation, the structural conditions of the rules distributing this allophonic variation are (3), (4), and (5).



(An  $x$  at the end of a line excludes syllabic association. So, for instance, the segment in (3) neither follows anything in its syllable nor belongs to a preceding syllable.) These are equivalent to (3)', (4)', and (5)'.



$$(3)' \quad (\sigma [-\text{cont} \atop +\text{stiff v.c.}] X)$$

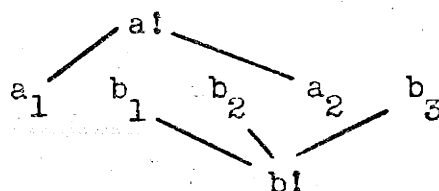
$$(4)' \quad (\sigma X [-\text{cons}] \left[ \begin{array}{c} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{cor} \end{array} \right] Y)$$

$$(5)' \quad (\sigma X [-\text{cons}] (\sigma \left[ \begin{array}{c} -\text{cont} \\ +\text{stiff v.c.} \\ +\text{spread glottis} \\ +\text{cor} \end{array} \right] ) Y)$$

In these examples,  $a_1 a_2 \dots a_n$ , of which constituency in the sense of (1) is being predicated, are segments.  $X$  and  $Y$  are variables, standing for any number of adjacent  $a_i$ 's, and allowing in this case for exhaustive analysis of syllables. Thus we stipulate that stops must be syllable-initial in (5)' by omitting a variable after the left parenthesis.  $\alpha$  has the value  $\sigma$ , the only label allowed nodes at this level of representation. (See, however, Duncan (to appear).) (For another example of  $\alpha = \sigma$ , see McCarthy (to appear).)

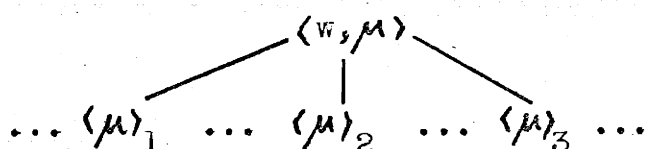
I draw the reader's attention to the fact that  $a_i$  and  $a_{i+1}$  may logically be separated by more than brackets. Suppose  $\underline{a}$  and  $\underline{b}$  were mutually exclusive natural classes. Then  $d_a$  would operate on, say,  $[a_1][b_1][b_2][a_2][b_3]$ , leaving  $a_1 a_2$ . There are, in fact, theories of discontinuous "non-linear" phonology (Clements 1976, Vergnaud 1977) in which it is convenient to build word-internal trees over segments belonging to the same natural class and in which segments  $a_1$  and  $a_2$  above might end up being a constituent, as in (6):

(6)



For example, Vernaud (1977 and to appear) makes the following ingenious proposal (here grossly simplified) for eliminating variables in word-level phonology. Let every member  $\mu$  of a natural class  $\underline{\mu}$  in a terminal string  $\mathcal{T} = w$  (i.e. the word) be dominated by a node  $\langle \mu \rangle$ . Let the maximal sequence  $\langle \mu \rangle_1 \langle \mu \rangle_2 \dots \langle \mu \rangle_n$  in turn be dominated by a node  $\langle w, \mu \rangle$ . Now Vergnaud is in a position to define an apparently discontinuous harmony process, for instance, over the node  $\langle w, \mu \rangle$ .

(7)



Here  $\mathcal{A} = \dots \langle \mu \rangle_1 \quad \dots \langle \mu \rangle_2 \quad \dots \langle \mu \rangle_3 \quad \dots$

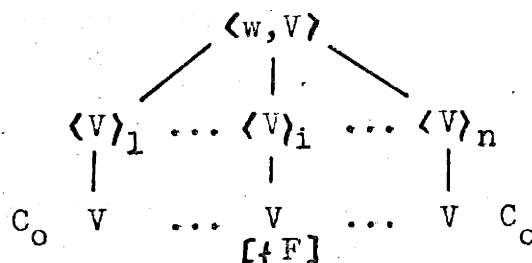
$d_\mu(\mathcal{A}) = \quad \langle \mu \rangle_1 \quad \quad \langle \mu \rangle_2 \quad \quad \langle \mu \rangle_3$

$\omega = [ \langle w, \mu \rangle \langle \mu \rangle_1 \quad \langle \mu \rangle_2 \quad \langle \mu \rangle_3 ]$

and we write  $(\langle w, \mu \rangle \langle \mu \rangle_1 \langle \mu \rangle_2 \langle \mu \rangle_3)$ . In this instance  $\mathcal{A}$  has the value  $\mu$ , since we want  $d_\mu(\mathcal{A})$  to give the sequence

$\langle \mu \rangle_1 \langle \mu \rangle_2 \dots \langle \mu \rangle_n$ .  $K = \langle w, \mu \rangle$ .

In Nez Perce, to give a more concrete illustration (see Aoki (1966), SPE (p. 377)), the diacritic feature  $[+F]$ , originating from any  $V_i$  that happens to possess it, infects every other  $V_j$  under  $\langle w, V \rangle$ , where it consummates the complex alternation characteristic of harmony in this language.



$\alpha$  has the value  $\langle w, V \rangle$  and  $a$  has the value  $V$ . We can write the domain of the rule as below.

$$(8) \quad (\langle w, V \rangle^X \quad V \quad Y) \\ \quad \quad \quad [4F]$$

This says merely that the sequence inside the parentheses must be a  $\langle w, V \rangle$ .

My point is that tree geometry conditions phonological rules at this level of structure precisely as it does at the level of the syllable.

It now seems a fair bet that the nodes provided by phrase structure could define the domains of higher-level rules in analogous ways. One would be surprised and disappointed if they alone among nodes, broadly considered, could not.

I believe that they can. I do not wish to say, however, that any kind of syntactic conditioning should necessarily be written into the rules subject to it. Even if the junctures involved could be characterized in terms of independently necessary boundaries, it would be a mistake to put such a characterization into the rules affecting the flanking segments. This is because quite often there is a list of rules all subject to the same condition. (Selkirk, to appear.) In Anlo-Ewe, for instance (Clements, to appear), there are (at least) three tone sandhi rules, all of whose domains of application are defined in terms of the same structural configuration. Pretty clearly, writing the same condition  $n$  times in  $n$  rules amounts to admitting that its repeated occurrence is an accident, whereas coming up with a way to factor it out and predicate it of all  $n$  says that its repeated occurrence is

part of a larger generalization.

This point is in fact already a point against supposing that terminal juncture symbols, like word boundary, could be the real factors conditioning selective sandhi, in any way at all. Word boundaries obviously show up in normal phonological rules, because, in the standard theory, they're phonological units. But then, if we let them condition high-level sandhi, what stops us from referring to their presence repeatedly and identically in a list of rules? (See the French case, below.) On the other hand, if the real conditioning factors have to do solely with pre-terminal structural information, not much needs explaining, since this we have no independent reason to want to terminally encode. If phonological and syntactic conditions cannot be mixed, the syntactic conditions cannot be specific to each phonological rule.

The form of these conditions is simply that of the rules we have already seen, taken to one remove of abstraction. Let us imagine a  $\varphi$ -juncture J,  $\varphi$  from the set of lexical units  $\underline{U}$  (see Part I), which a rule or list of rules at a level higher than  $\varphi$  is going to relate features on either side of. (To breach a morphological juncture, for instance, we need at least a word-level rule.) I'll symbolize  $\epsilon$  the two immediate  $\varphi$ -units,  $\psi$  from the set of levels  $\underline{L}$ , on either side of J and containing at their far right and far left segments bearing these features.  $\varphi$  is not necessarily the same as  $\psi$ : It turns out (below) that we'd like, for instance, to be able to set  $\psi$  equal to f, though there is presumably no f-juncture

as distinct from the w-juncture.

Now what we want to do is predicate constituency, in the sense of (1), of strings containing  $\varepsilon\varepsilon$  by way of constraint on the rule or rules affecting them. For instance:

$$(9) \quad X \left( \begin{smallmatrix} \varepsilon & \varepsilon \end{smallmatrix} \right) Y$$

X and Y are variables, standing for any number of  $\psi$ -units. By using the variables, I intend (9) to be an exhaustive analysis of a k-sized string ( $k \in \mathbb{L}$  and higher than  $\psi$ ), i.e. it's a k-level condition.

According to (9),  $\varepsilon\varepsilon$  must be a  $\kappa$ . Suppose  $\kappa = f$  (the set of phrasal nodes  $\begin{bmatrix} \beta^N \\ \delta^V \end{bmatrix}^b$ ,  $b > 0$ ) and  $\varepsilon = w$ . Then we'll interpret (9) as saying that a pair of adjacent words must be a syntactic constituent in order for some rule(s) to apply. As I'll show below, (9) is now precisely what is needed for the Hebrew case.

(Note that there is presumably no node ultimately dominating segments separated by full stop. The theory is predicting, then, correctly to my knowledge, that selective sandhi between successive sentences in an utterance will not exist.)

## THE FRENCH EXAMPLE

My first example is French liaison. The term liaison refers to the pronunciation, under certain syntactic conditions, of word-final consonants before word-initial vowels. The usual assumption is that underlying final consonants delete elsewhere--when a vowel does not follow or when the syntactic conditions are not met. It is these conditions which get most of my attention below, and not the phonological rule they govern. Surprisingly, though, the theory I'll develop to handle the former has a few interesting things to say about the formal nature of the latter.

The example of liaison is unfortunately not entirely happy, because the proper boundaries of the phenomenon are not so easy to discern through the sociological fluff which surrounds it. Generally, the less casual the conversation, the more final consonants are audible. Usually people who think about it respond to this fact by speaking of "styles" of liaison, all of which a given French person is in command of and chooses among depending on social context. Fouché (1959), for example, precipitates a vast range of liaison phenomena into three discrete styles--la conversation courante (Style I), la conversation sérieuse et soignée (Style II), and le style soutenu (Style III), named in order of increasing number of syntactic liaison contexts. (This classification is accepted by Selkirk (1972).) The set of liaison contexts char-

acteristic of style  $n$  is a proper subset of the set of liaison contexts characteristic of style  $n+1$ , which is to say that no increment in formality results in the loss of any liaison context. On the contrary, a core of pan-Gallic, general-purpose liaison is successively added to, yielding the more rarefied styles. (Remember, though, that this effect is achieved, in most accounts, by allowing a rule of final consonant deletion to apply less frequently.)

Now speakers of each style have a certain amount of latitude in rendering it. For a given style  $k$ , liaison in a syntactic context  $C$  is either obligatory, optional to various degrees, or totally out of the question. Liaison in  $C$  may be required or prohibited because it is required or prohibited by all styles, i.e. by French; or it may be required or prohibited because of the stipulated characteristics of style  $k$ . Now if liaison in  $C$  is merely allowed, with more or less enthusiasm, there always exists a higher style in which it is obligatory and/or a lower style in which it is forbidden. It would be entirely appropriate, once we've decided to idealize discrete styles in which liaison is not optional, to eliminate the remaining optionality in  $C$  by replacing  $k$  with  $k'$ , like  $k$  save that liaison in  $C$  is prohibited, and by setting up a higher style  $k''$ , like  $k'$  save that liaison in  $C$  is obligatory. However, in view of the fact that infinitely many styles would be required to eliminate optionality in all contexts, it would also be absurd. But now we begin to wonder about the idealization to the first three styles.

I am not pushing for a squish, at least not a linguistic one. My point is that any discrete stylistic treatment of liaison is unfair. There is a set of liaison contexts which it would be un-French not to observe. This set ("Style I") is the real linguistic phenomenon, the common denominator of which is unconsciously known in the form of a rule (to be brought to consciousness below) by every speaker. In addition to this piece of knowledge, every speaker is also aware of a certain social pressure to make more liaison in stuffier social contexts. To what extent and how consistently it is actually performed depends mainly on the speaker. There is some absorption through normative institutions (e.g. the schools) of ideas about "correct" liaison in the formal styles. So, for instance, educated speakers may have been explicitly taught that, for poetry reading and such events, liaison is supposed to be made before a post-nominal adjective: des idée[z] absurdes. This "rule" is very much like a rule of spelling. Both are explicitly taught to some fraction of the population; both may or may not be learned, or if learned remembered, or if remembered observed; and both have zero linguistic significance.

Liaison, however, is not fortunate to have a well-established, consultable norm, doing for it what the dictionary does for spelling. Consequently it is the case that there is almost no agreement about the facts of liaison in non-conversational styles. Beyond such simple cases as the post-nominal adjective, very few people have much idea about where to



put their liaison, simply because there are no real, productive rules. They know, "I'm supposed to be making more liaison," and they know this with more certainty as the situation gains in formality. Some socially conscious speakers develop their own elaborate routines for non-conversational liaison, which they may stick to quite consistently; but seldom do they have genuine intuitions extending to unusual syntactic contexts, and their routines are strictly their own. I mention as an analogue the amazingly involved and varied private routines that many literate speakers of English have developed for avoiding, or flaunting, sexist choices of unmarked anaphoric pronouns. It is not clear in what sense this kind of linguistic behavior, existing solely for social reasons, represents something that is part of the language. The rules responsible are far from trivial, but, I would say, not interesting to a linguist qua linguist.

In sum, then, I think that the non-conversational styles, to the extent that they exist, are artificial and normative. To idealize to discrete styles is misleading, just as it is misleading to say that there is a discrete style in English in which people don't use contractions, don't strand prepositions, or don't palatalize final dentals (see Part IV). Rather, the rule-governed compulsion to drop the final consonant, contract, strand, and palatalize is variably offset by the variably effective social sanctions against it. Not sharing this view are Fouché (1959), Selkirk (1972, 1974), Rotenberg (1975, 1976), and Postal and Pullum (1977).

I'm going to go on now and review what has been said about the syntactic conditioning of conversational liaison.

There is only one clear idea in the literature on this subject. It enjoys wide currency and is, to my knowledge, unchallenged in print. Due originally to Milner (1967), it is accepted by Schane (1968), Dell (1970), and Selkirk (1972):

- (10) "...the phonological phenomena characteristic of liaison operate when just one word boundary, #, separates one word from the next." (Selkirk, p. 208.)

These two words are spoken of as "being in a liaison context."

This is an interesting, powerful, and elegant theory, worth examining closely. Let us in fact examine it closely. The idea is that there is a rule on the order of (11). I temporarily simplify.

- (11)  $C \rightarrow \emptyset / \_ \# C$

Suspending for the time being our opinion of rules like this, we should ask exactly where a single # is allowed to appear. Recall, first of all, the SPE conventions on boundary placement which I outlined in the introduction. Given these, we are in a position to find out in what syntactic circumstances a sole # may stand. Let us imagine a pair of adjacent words A and B.

1. Suppose A and B both belong to lexical categories.

Then A will be [# A #] and B will be [# B #]. We have

- (12) [# A #] ... [# B #]

where the dots stand for possible intervening brackets.

There is no chance of redemption by the boundary-telescoping convention BC II; two word boundaries permanently separate A from B.

2. Suppose A is non-lexical and B is lexical. Then A is [A] and B is [# B #].

i. Now suppose A is dominated by some node  $\alpha$  that does not also dominate B. Then we have:

(13) [ $\alpha$ #... [A] #][# B #]

Once more ## separates A from B.

ii. If B is dominated by some node  $\alpha$  not also dominating A, BC I gives

(14) [A] ... [ $\alpha$ #[# B #]... #]

which BC II improves to

(15) [A] ... [ $\alpha$ #[ B ]... #]

But A must be dominated by something,  $\beta$ ; as the preceding shows, any  $\beta$  dominating A must also dominate B if  $\beta$ 's # is not going to appear to the right of A.

(16) [ $\beta$ # ... [A] [ $\alpha$ # [B]... #]... #]

Put another way, a single # separates A and B if and only if the first node dominating A dominates B.

This relation between A and B is similar to the idea of C-command (Reinhart 1976): A C-commands B if and only if the first branching node dominating A dominates B. For convenience in the following, I will invent a relation IC-commands (immediately C-commands).

3. Suppose A is lexical and B is non-lexical. Then, by

identical reasoning, if and only if B IC-commands A, a single # intervenes.

4. Suppose A and B are both non-lexical. Then A is [A] and B is [B].

i. If A is dominated by a node  $\alpha$  not dominating B, we have

(17)  $[\alpha \# \dots [A] \#] \dots [B]$

If any  $\beta$  dominates B without dominating A, another # appears before B. Therefore a single # separates A and B if and only if B IC-commands A.

ii. Similarly, if B is dominated by a node  $\beta$  not dominating A, one # intrudes between A and B if and only if A IC-commands B.

iii. If some  $\gamma$  immediately dominates both A and B,

(18)  $[\gamma \# [A] [B] \#]$

no #'s appear between them. Note that A and B IC-command each other.

What (10) says, then, is that two adjacent words are in a liaison context when and only when one, a non-lexical item, IC-commands--is immediately dominated by a node dominating--the other. We may now look and see if this is true.

I briefly survey the basic liaison contexts. Final consonants are heard between:

(19)(a) a pre-nominal adjective and its noun

(b) a pre-adjectival adverb and its adjective

(c) an auxiliary and its verb

(d) a copula and its object

- (e) a determiner and what follows
- (f) a pronoun ("clitic") and what follows
- (g) a preposition and what follows

E.g.:

- (19)(a)' un grand^orang-outang
- (b)' vraiment^idiot
- (c)' ont^été
- (d)' est^absurde
- (e)' un^homme
- (f)' en^est
- (g)' en^été

I use without comment from now on the ligature "^" to indicate liaison, and the solidus "/" to indicate no liaison.

Often, when it is irrelevant, I will indicate neither.

The first thing to notice is that the contexts in (19) fall into two classes. (i) There are words--prepositions, determiners, and pronouns--which always make liaison. This is to say that for a given preposition, article, or pronoun of the form /XVC/, one always finds [XVC] V, never [XV] V. (There are highly significant exceptions, to which I return.) (ii) In all the other cases, [XVC] V alternates with pre-vocalic [XV] V. (I assume that words which always appear as [XV] in fact possess a final consonant at no level of representation.)

- (20)(a) un bon/et gros oignon
- (b) vraiment/idiot et ridicule

- (c) ont<sup>?</sup>/astucieusement avoué
- (d) est<sup>?</sup>/absolument absurde

I put off discussion of the first class for a while. Provisionally observe, however, before leaving the subject, that no syntactic generalization covering these cases is going to be possible, or, for that matter, necessary: We could very easily conclude that these words themselves are inducing the liaison, much as Irish a 'his', as opposed to a 'her', induces lenition; and that (ii) the categories to whose right liaison unstoppably occurs are all non-lexical. (i) and (ii) are not unrelated.

As for the second class, it seems obvious that the word on the left need not belong to a non-lexical category. I might approach an even stronger statement by noting that it is not in the least clear that what I informally refer to as the auxiliary and the copula must in fact be represented in French as belonging to formal categories distinct from V. See, on this, Emonds (to appear). The word on the right, due to the right-nuclear phrase structure of French, is (at least in the simple examples in (19)) never non-lexical. Therefore it seems to be a mistake to require, as the boundary theory does, that at least one of the words in a liaison context be non-lexical.

(Selkirk (1972) is quite alive to the problems posed by (19)(a) and (b), on the assumption that adjectives and adverbs, but not auxiliaries and copulas, belong to lexical categories. To deal with (b), it is assumed that adverbs are really non-

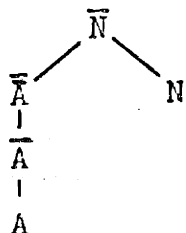
lexical. To deal with (a), a thornier problem, a special rule deletes a word boundary.

(It is interesting to observe the general philosophy of stylistic liaison in this work. The idea is that (10) is always true, holds good for all styles (of which I am only considering one). But the styles are obviously different. Therefore the boundaries must be different. And in fact, to give the good results, style-specific readjustment rules work to insert and delete the respective wanted and unwanted boundaries, starting with the basic installation given by SPE conventions.

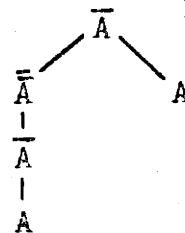
(One begins to fear for the content of (10).)

Now it is undeniable that one word IC-commands the other in all cases.

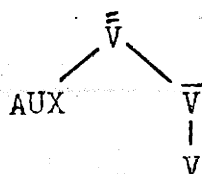
(19) (a) "



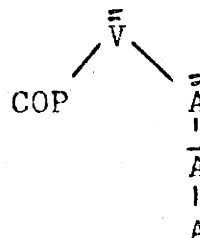
(b) "



(c) "



(d) "



But note that, in these simplest of examples, the two words also C-command each other, and each C-commands only the other, which is merely an involved way of saying that the two of them exhaust some constituent. We might easily entertain sus-

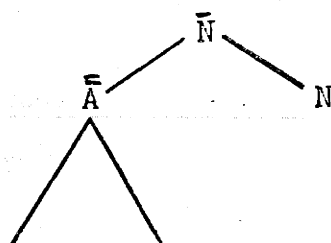
picians that these facts are trying to tell us that something like (21) is actually governing liaison.

(21)  $X \left( \begin{smallmatrix} w \\ f \end{smallmatrix} w \right) Y$

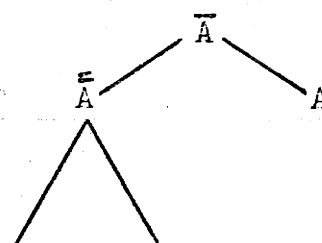
(21), a condition on sentence-level rules, says simply that the two phonological domains flanking the w-juncture across which a rule might apply must be a constituent unto themselves.

To say, as we are now doing, that a liaison consonant is heard at the end of a word if and only if (phonological conditions being favorable) it and the next are a constituent is, however, too strong. What if, in (19)(a)"-(d)", we try expanding the one expandable sister?

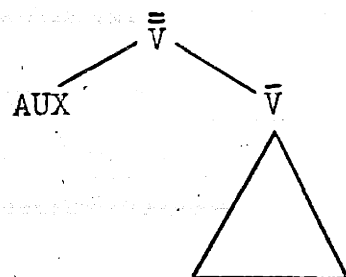
(22)(a)



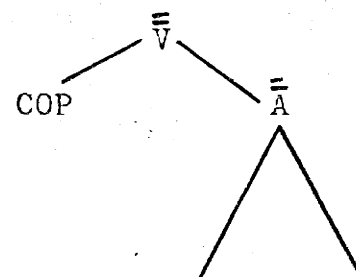
(b)



(c)



(d)



(a)' un beau et gros<sup>o</sup>ignon

(b)' complè<sup>e</sup>ment, v<sup>a</sup>raiment<sup>absurde</sup>

(c)' ont<sup>?</sup>/astucieusement avoué

(d)' est<sup>?</sup>/absolument absurde

What happens is that liaison fails only when the com-



plexity is introduced on the right--i.e. when the word to the right of the liaison consonant does not end the constituent.

Notice, too, examples like the following.

(23) (a) un gros/et bel oignon *special liaison form of 'beau'*

(b) Il est influent/avocat et législateur à la fois.

(I assume Ross' (1967) account of coordinate structures, ac-

cording to which the conjunction and the second conjunct are themselves a constituent. (See Part I.)) But:

- (24) (a) un gros^oignon
- (b) un très gros^oignon
- (c) un absolument, incroyablement gros^oignon

And:

- (25) (a) trop/ou pas assez intelligent
- (b) à la fois trop/influent et vieux

But:

- (26) (a) trop^absurde
- (b) vraiment trop^absurde
- (c) sans doute indéniablement trop^absurde

In an example like (27),

- (27) Jean est très intelligent et modeste.

{ liaison appears before intelligent only if the bracketing is as in (27)'(a), not (27)'(b).

- { (27)'(a) [très intelligent] et modeste
- { (b) ? très [intelligent et modeste]

In the verb phrase, parallel effects obtain, though for some reason they are never nearly as clear-cut. Liaison becomes a good deal less natural before a complex verbal complement.

- (28) (a) Nous allons^atterrir.
- (b) Nous allons^?/atterrir à Paris dans un instant.

The theory I'm going to stick with is formalized in (29).

- (29) X ( <sub>f</sub> Y w w ) Z

(29) says only that the unit to the right of the w-juncture

breached by liaison must finish a constituent that also contains the unit to the left of it. This handles everything I've mentioned. Notice, incidentally, that a boundary theory predicts exactly the wrong results in (22)(a)'-(d)', and is incapable of distinguishing (23) from (24), (25) from (26), (27)'(a) from (27)'(b), or (28)(a) from (28)(b).

The rule of liaison subject to (29) must evidently be something like (30).

(30)  $C \rightarrow [+unit] / \text{---} V$

In (30) I make use of two formal gimmicks to get the right results. The first, the feature  $[+unit]$ , which opposes all units ( $[+unit]$ ) to zero ( $[-unit]$ ), is due to Morris Halle. The second is conventional elsewhere-case alpha-switching (Kiparsky 1973): Every rule

(31)  $A \rightarrow [\alpha F] / \text{in some environment}$

actually abbreviates

(31)'  $A \rightarrow \begin{cases} [\alpha F] / \text{in some environment} \\ [-\alpha F] / \text{elsewhere} \end{cases}$

What I intend by (30), then, is that consonants become  $[-unit]$  --delete--in environments other than the specified one, i.e. before C or zero, or before V if (29) is not met too. The result, observe, is that the rule (30) saving liaison consonants from deletion applies vacuously where the liaison consonant actually surfaces. Speaker intuition evidently agrees that something seems to be "happening" in liaison environments (i.e. not elsewhere), even though the audible consonant presumably ascended straight from the deepest representations.

(30), final consonant saving, is consistent with the feeling that deletion, not no deletion, is the unmarked case.

I think we would do well at this point to take quite seriously the formal awkwardness of (30). Why is it that we are forced into such an oddly contorted solution? The reason is twofold: (i) It turns out to be formally easy, given the theory I'm working with, to describe the environment where liaison is heard. The environment where liaison is not heard is best described simply as the complement of (29). (ii) I've assumed that liaison consonants start in the deepest representations and are deleted wherever they are not present on the surface. From (i) and (ii) it follows that we must arrange for deletion not to apply in the easy-to-describe liaison contexts.

(i) I will not question. It is simply a fact, though about a theory I wish to maintain. (ii), however, is a questionable assumption. True, it has the weight of tradition behind it. Nevertheless, there is a quite competitive account according to which liaison consonants are inserted: See Klausenburger (1974, 1978). I urge the reader to set aside any accumulated theoretical prejudice and consider it. At first glance it is difficult to imagine what sorts of empirical evidence there might be to distinguish the insertion theory from the deletion theory. It does exist. Unfortunately, the considerations adduced by Klausenburger, a natural generativist, are identical in spirit and nearly point-by-point to those cited by Vennemann in favor of underlying a plus an

a → an rule (recall Part I): developmental priority, frequency of occurrence of alternating forms, and so on. These seem weak to me.

Note first that on the insertion theory we can simply insert the liaison consonant in the liaison context (29). The mechanics of this insertion are fairly trivial--basically an a → an rule on a grander scale. We might imagine that each word  $\varphi$  which makes liaison has associated with it a liaison consonant L (henceforth L-consonant), just as a has associated with it what amounts to a liaison consonant n. So we might write:

$$(32) \quad \emptyset \rightarrow L / \varphi \text{ \_\_\_\_ } V$$

This rule is subject to (29) in a straightforward way.

As a second and probably less equivocal example of the mechanism I have in mind, consider the familiar problem of strong verb ablaut in English.

I'm simply going to assume that SPE is wrong in trying to push the strong verbs through various parts of the vowel shift. I assume further that no independently necessary rule or rules derives or derive sang, wove, took, etc. from sing, weave, take, etc., and that no phonological generalization uniting the considerable range of alternations is even possible. (See, however, Halle 1977.) On the other hand, we evidently don't want to say that sang is merely suppletive for singD, and so on, because we miss thereby the fairly good generalization that the verb stem, apart from the varying vowel, is constant in the present, past, and also participial

forms. In some fashion, then, we clearly want to make just the vowel suppletive.

A natural idea is to list, in the lexical entry for each verb, the alternating vowels, and to invent labels for them:

- (33) /siŋ/  
 $V_{pst}$ : [æ]  
 $V_{ppl}$ : [ʌ]

Having done this for all the strong verbs, we can of course write general morphological rules using the labels  $V_{pst}$  and  $V_{ppl}$ .

- (34)  $V \rightarrow V_{pst} / \text{--- } C_o D$

- (35)  $V \rightarrow V_{ppl} / \text{--- } C_o EN$

Something along these lines seems necessary in this case, as far as I can see. Similar examples of partial suppletion will occur to the reader in the languages that she knows.

Here it is obviously absurd to say that the underlying form /siŋ/ in any sense contains the alternating vowels [æ, ʌ].

What I'm suggesting is that the same kind of lexical structure is appropriate for French. Each word  $\varphi$  which participates in liaison has listed with it an associated liaison consonant, labeled L, which we can then insert by rule.

- (36) /gro/

L: [z]

It is undesirable to list both /gro/ and /groz/ as free variants, and then rule out /groz/ before a consonant, for the

same sort of reason that it is undesirable to list /sig/, /sɛŋ/, and /sɛŋ/: We'd be losing the generalization that in every case the prevocalic form differs from the preconsonantal one only in being one consonant longer. Note that here, unlike the English case just cited, it is not absurd to say that the underlying form /gro/ also includes the final /z/; but it is not equivalent, since the good consequences (above and below) of the insertion rule do not follow. In this respect the situation is precisely analogous to the a/an case of Part I.

As a case intermediate in lexical saturation between the English a/an example and the French example I might mention the Classical Greek movable consonants. I take no stand here on whether these are inserted or deleted; my point is just that these alternations, while not merely suppletive, are not phonological in nature either and must be lexically governed, in spite of the fair number of morphemes involved.

There exists a small list, whose contents vary slightly depending on dialect, of morphemes of which prevocalic variants having final nu or sigma (two of the three final consonants generally possible in Greek) occur. These must simply be learned: No phonological generalization picks out the morphemes to which the rule carrying out the alternation applies, nor is one to be expected. A true phonological generalization specifies the environment of the movable consonant--namely, everywhere but before another consonant, including sentence-finally. In this it differs from liaison.

Also, there is no evidence that movability of sigma or nu is at all affected by constituency.

Movable nu appears (i) after estí 'is' (éstin állos 'there is another', ésti theós 'there is a god'); (ii) after the third person singular in -e (élegen ekéinon 'he said that', élege lógon 'he said a word'); and (iii) after most words in -si. -si generally is or ends an inflectional morpheme of one sort or another, marking (e.g.) the dative plural (pá:sin élegen 'he spoke to everybody', pá:si légo: 'I speak to everybody'), the locative (athé:ne:sin ê:san 'they were at Athens', athé:ne:si polemôusin 'they fight at Athens') or the third person plural (légousin emoi 'they speak to me', légousi soi 'they speak to you'); thus the list of word-final morphemes in -si is actually small.

Movable sigma occurs (i) after ek 'out of' (eks agorá:s 'out of the marketplace', ek póleo:s 'out of the city'), and (ii) after hoúto: 'thus' (hoúto:s epoiei 'he acted thus', hoúto: poiéi 'he acts thus').

For the Greek case a series of lexical variants must surely be listed in some way. This conclusion in itself does not allow us to choose between an insertion account and a deletion account of the relation between the variants. Formally speaking, the two are merely restatements of one another; a complexity issue does not arise. In the same way, note that the arrangement in (32) is neither simpler nor more complex than a deletion rule operating on ad hoc underlying consonants. Fairly patently, the necessary machinery is even between deletion and insertion.



However, the number of exceptions to the deletion rule is absolutely enormous, and there is nothing un-French about them: avec, tous, oeuf, six, gaz, baleine, mec, homme, auto-bus, etc., etc. These words are neither borrowed, odd-sounding, nor especially recent additions to the language. They possess no other exceptional features which might be construed as concomitant. They are never mistakenly subjected to the loss of their final consonants.

If there is no deletion rule, of course, they are not exceptions, any more than banana is an exception to the a/an rule. We need not deal with them by giving them negative rule features or underlying protective final schwas, as have been done. On the contrary, it is the words which have genuinely movable finals that are exceptional. I noted that the proposed deletion rule shows no sign of losing its exceptions. By contrast, the exceptional insertions are definitely losing in both strength and numbers. Klausenburger notes that "optional liaison" (on which more below) is now heard less than fifty per cent of the time from many speakers, and the L-consonants of many words are increasingly finding younger speakers who do not recognize them at all: devant, trop, ont, etc.

Another important fact connected with L-consonants is their syllabic membership. Note first the related fact that pause does not block liaison.

- (37) (a) \*[me] yeux
- (b) [mez] yeux
- (c) \*[me]...yeux

If it were true merely that pause blocked liaison, (37)(c) ought to be acceptable. The real generalization is that the L-consonant must begin a syllable. This is the well-known enchaînement.

(38)(a) \*[mez]...yeux

(b) ?[me]...[z]yeux

(38)(b) is, surprisingly, not so bad. This too would be difficult to understand if liaison paid no attention to pause. It is wrong, however, to think that all final consonants must begin syllables. The "exceptions" to liaison survive quite well before pause.

(39) Avec...(un) quoi?

Therefore the fact that L-consonants must begin syllables is not part of a larger truth about French syllable structure. (I don't believe that these exceptions are to be blamed on underlying final schwas, as in Dell (1970); but even if they were, syllabification would undoubtedly take place after their disappearance.) Likewise, we could not do a general resyllabification, à la Kahn, of finals, whereby they would end up tautosyllabic with the next vowel, and then condition liaison on it:

(40) \*Ave...[k]un quoi?

(I am assuming, of course, that one thing which does not admit of lexical exceptions is syllabification. However, even if we could list avec as an exception to it, we would be wrong. A normal pronunciation of avec un quoi has syllable-initial [k].

On the other hand, once the theory has distinguished

an insertion analysis, making liaison essentially morpho-

This last fact raises an interesting question. Given

seems entirely appropriate.

strictly artificial. A special insertion rule to cover it

continue to assume that its availability in higher styles is

associated with the zero plural morpheme attached to nouns. I

insertion account: There simply is no L-consonant [z] as-

liaison (des idées absurdes). This problem vanishes, on the

entirely. Consider, for instance, the problem of post-nominal

of labor in getting ordinary conversational French rid of it

sent in all underlying representations, makes for a good deal

L-consonant heard in one of the higher "styles" must be pre-

that the deletion theory, together with the assumption that an

own. It does, however, make things extremely simple. Note

son in what follows, though it is not absolutely crucial to my

(32). I will continue to accept an insertion theory of liai-

For no very good reason, I will choose (42) and keep

(42)  $\begin{array}{c} \text{L} \\ \text{L} \end{array} \begin{array}{c} \text{L} \\ \text{L} \end{array}$

in Part I.

(ii) We install a filter, much like the one we needed for an

(41)  $\emptyset \rightarrow L / \emptyset \text{ — } V$

left word edge.

Two possibilities occur to me. (i) We insert L actually at

the inserted consonants with respect to their syllabification.

which are not, we are in a position to do what we like with

L-consonants, which are inserted, from run-of-the-mill finals,

nemic, we might expect to find L-consonants associated with morphemes, as in the Greek case, rather than with words.

This may actually be the right conclusion. Selkirk (1972) catches liaison in the act of evidently applying word-internally, across (in boundary-theoretic terms) what must be  $\tau$ , as illustrated by the alternations [de] ~ [dez], [me] ~ [mez], [su] ~ [suz] in the (derivational) prefixes dés-, més-, sous-:

(43)	débloquer	désassembler
	décroiser	désinfecter
	défavoriser	désorienter
	méfait	mésaventure
	mécompte	mésestimer
	médire	mésintelligence
	souligner	sousestimer
	soulever	sous-alimenté
	soutenir	sous-officier

(Selkirk's examples, p. 304.) Also across word-internal #, though the evidence seems remarkably weak: vit ~ vivons, dort ~ dormons, etc. (See Schane 1968.) Facts like these call for an emendation of the deletion rule in (11):

$$(44) \quad C \rightarrow \emptyset / \text{---} \left\{ \begin{array}{c} \# \\ + \end{array} \right\} C$$

But we need not do anything like this. The conclusion is simply that the units  $\varphi$  which have associated L-consonants are morphemes.

Another kind of example bearing on the same issue is the adverb in -ment. Some speakers pronounce the [t] in liaison contexts; others have [mã] everywhere. But there are no speakers who say, e.g., probable[mãt] V, but spéciale[mã] V. If L-consonants were associated with words,

we might expect variation like this.

To press the same point a little, note that liaison does not apply across zero, i.e. no "boundary." Suppose we accept a deletion account. The relevant examples are then piston, actuel, agnostique, exiger, hebdomadaire. Suppose we accept an insertion account. The relevant examples are nuage, Saône, naïf, douane, créance. Even though, in all ten cases, a syllable break presumably intervenes, we do not find CC becoming ØC by deletion or VV becoming VCV by insertion. On the first theory, this state of affairs is slightly puzzling. Since it follows from nothing in particular, we must deploy specified boundaries: (44). But on the second theory, it follows right away from the fact that a syllable is not a unit which can be listed in the lexicon.

At this point I should remind the reader of the advantages gained by freeing the abstract structural conditioning (29) from the nuts and bolts of the actual rule of liaison (32). First, remember that the conditioning is purely syntactic. (32) applies word-internally unconstrained by it. Thus, writing something like

(45)  $\emptyset \rightarrow L / X ( \text{ } _f Y \epsilon_1 \text{ --- } \epsilon_2 ) Z, \epsilon_1 = \varphi, \epsilon_2 = VX$   
crucially misrepresents the nature of the process. It is simply not true that liaison applies only across w-juncture.

Second, there is no other way to subject a list of rules to the same condition, at least without making its appearance in rule after rule look like some sort of amazing accident. It is in fact repeatedly true (Selkirk, to appear)

that configurational sandhi contexts have some generality. For this reason we'd like to be able to factor the context out of individual structural descriptions.

Somewhat unfortunately for my purposes, French does not, despite Selkirk (1972 and to appear), constitute an illustration of this necessity. It is the case (Selkirk 1972) that several processes other than liaison seem to apply in the same context expressed by (29). However, on closer consideration, it turns out that liaison simply feeds them, in a quite straightforward manner. It is even unnecessary to have them apply across w-juncture at all. I'm going to go into it here because I think it's an interesting piece of phonology.

The second process to evidently respect (29) is nasalization. Nasal vowels in French appear before consonants or word edge, e.g. bon, bonté [bɔ̃, bɔ̃te]. Nasal vowels often participate in alternations with sequences of non-nasal vowel plus nasal consonant: bonheur, bonne [bɔ̃nɛr, bɔ̃nɛ]. Writing a nasalization rule to account for this sort of thing is a traditionally attractive idea (but see Tranel 1977). The rule looks something like (46).

$$(46) \quad V N \Rightarrow \begin{matrix} 1 \\ [+nas] \end{matrix} \emptyset$$

(46) allows us to purge underlying representations of all nasal vowels, which is supposed to be an important economical measure.

The case for an active rule, though not necessarily (46), seems strengthened by the existence of facts like the follow-

ing.

- (47) (a) C'est un b[ɔ̃] copain.  
 (b) C'est un b[ɔn] ami.  
 (c) Est-ce b[ɔ̃] ou est-ce mauvais?  
 (d) C'est b[ɔ̃].

Although lexical correspondences could handle morphological alternations like

- (e) b[ɔ̃]té  
 (f) b[ɔn]heur

they clearly are not up to facts like those in (a)-(d), which show that the context is infinitely variable. See Part IV.

The drift of (47) is that nasalization fails exactly when it would delete a consonant that liaison inserts. Clearly we want to say that the [n] of bon is an L-consonant. Note (48).

- (48) (a) C'est un très très très b[ɔn] ami.  
 (b) Ce sera b[ɔ̃] après demain.  
 (c) C'est b[ɔ̃] en été.

To make this idea work, however, we have to make /bɔ̃/ the underlying form. Liaison produces [bɔ̃n], and what we are forced to conclude is a rule of de-nasalization gives [bɔn].

The result, which seems quite desirable to me, is that de-nasalization is simply a kind of by-product of liaison. I for one find I have no intuitions at all about what ought to be the directionality of rules like this. It seems to me that no one should be especially astonished by a de-nasali-

zation rule, except when prepared by previous nasalization accounts of the same facts. The rule takes the form (49). It is a word-level rule.

(49)  $V \rightarrow [-nas] / \text{---} [+nas]$

(32) and (49) work together to produce the examples of (43) as follows.

(47)'	(a)	(b)	(c)	(d)	(e)	(f)
Underlying:	$\tilde{V} C$	$\tilde{V} \sim V$	$\tilde{V}/V$	$\tilde{V}.$	$\tilde{V} V$	$\tilde{V} C$
Liaison:		N			N	
De-nasalization:		V			V	
	$\tilde{V} C$	VNV	$\tilde{V} V$	$\tilde{V}$	VNV	$\tilde{V} C$

(a)  $[b\tilde{3}]$  : word edge, preconsonantal

(b)  $[b\tilde{3}n]$  : word edge, prevocalic, liaison context

(c)  $[b\tilde{3}]$  : word edge, prevocalic, no liaison context

(d)  $[b\tilde{3}]$  : full stop

(e)  $[b\tilde{3}V]$  : morpheme edge, prevocalic

(f)  $[b\tilde{3}l]$  : morpheme edge, preconsonantal

One point about this analysis. Although extremely simple, it requires underlying nasals alongside of the plain ones. Is this bad? I think we ought to seriously question the idea that the optimality of a grammar has much to do with the size of the required phoneme inventory. I'm afraid I don't understand in what sense "eliminating" phonemes by writing phonological rules in itself constitutes a saving. It seems to me that optimality more clearly resides in the form of the rules of the grammar. The trend of much recent research is toward showing precisely this: We can, and in fact should in



specific cases, buy a sought-after impoverishment and simplification of the grammar at the price of an enrichment (though reasonably well circumscribed) of the lexicon.

I know of no indication that anybody learning French should want to resist setting up underlying nasals on universal grounds. On the contrary, any realistic alternation condition (Kiparsky 1974) will prevent a free-ride derivation by nasalization of words like dindon, quinze, oncle. Thus evidently we'll need underlying nasals anyway.

There are sometimes supposed to be distributional virtues to traditional deletion analyses of liaison and nasalization. Once we've postulated a level of structure at which French has its finals, it ought to be possible, as in so many cases, to regularize the distributional statements which we must at some point make by making them about this level of structure, whose regularity would be disturbed only in the ensuing derivation.

However, in the French case such advantages are not obvious. Note that on both insertion and deletion analyses, French must have final consonants and final vowels at all levels: avec, ami. Note that on both insertion and deletion analyses, nasal vowels must (accepting the alternation condition) occur initially, medially, and finally at all levels: onde, donc, chignon. As must nasal consonants, even when postvocalic: mot, emmener, rhum. It would be quite odd, of course, if the L-consonant associated with every liaison-making word ending in a nasal vowel turned

out to be a nasal consonant. On an insertion analysis, this would have to be an accident. But it is not true: grand, long, dans.

One more rule apparently applying in liaison contexts is what Selkirk (1972) refers to as FR-conversion. This rule is responsible for [r] ~ [r̥] ~ [r̥̥] alternations in every respect analogous to the -VN ~ V alternations above.

(50)(a) C'est un singulier fromage.

(b) C'est un singulier ami.

(c) C'est singulier en été.

(d) C'est singulier.

(e) singulier[ar]ite

(f) (no example)

(a) word edge, preconsonantal

(b) word edge, prevocalic, liaison context

(c) word edge, prevocalic, no liaison context

(e) full stop

(f) morpheme edge, prevocalic

(g) morpheme edge, preconsonantal

Following the same thinking, we want to make that [r] an L-consonant. Having inserted it, we readjust the underlying /e/ to [ɛ], as in Schane (1968):

(51) e → ɛ / — r

(Another rule backing ɛ before consonant plus high vowel is supposed to be responsible for the [a] of singularité (Selkirk, p. 348); this I accept for the purposes of my discussion. It must, of course, apply after (51).) (51) is a

word-level rule. Derivations:

(50)'	(a)	(b)	(c)	(d)	(e)
Underlying:	e C	e <sup>~</sup> V	e/V	e.	e V
Liaison:		r			r
E-lowering:		ɛ			ɛ
	e C	ɛrV	e V	e	ɛrV

Below I assemble the three rules whose effects are visible in liaison contexts. (52)(a) precedes and feeds (b) and (c).

- (52) (a) Liaison.  $\emptyset \rightarrow L / \gamma \text{ \_\_\_\_\_\_ } V$   
 (b) De-nasalization.  $V \rightarrow [-nas] / \text{ \_\_\_\_\_\_ } [+nas]$   
 (c) E-lowering.  $e \rightarrow \varepsilon / \text{ \_\_\_\_\_\_ } r$

I stress, at the risk of overinsistence, that in (52) I use no boundaries. In this practice I differ from other people (Schane 1968, Dell 1970, Selkirk 1972, 1974) who have thought about these processes. To instance the most recent treatment, Selkirk's (1972) assumptions force her to write the following rules:

- (53)(a) Liaison.
- $$C \rightarrow \emptyset / [ \# X \text{ --- } C_0 ( \begin{matrix} \uparrow \\ \# \end{matrix} C X ) \# ]$$

- (b) Nasalization.
- $$\left[ \underbrace{\# \text{ Y V N } ( \text{ \# } ) \text{ C X } ) \# }_{\substack{1 \quad 2 \quad 3 \quad \quad \quad 4}} \right] \Rightarrow \begin{matrix} 1 & 2 & \emptyset & 4 \\ & [+nas] & & \end{matrix}$$

- (d) ER-Conversion.
- $$\underbrace{[ \# Y ]}_{1 \ 2 \ 3} \text{ } \text{ } \text{ } \underbrace{ ( \# C X ) }_{4} \# 1 \Rightarrow \begin{matrix} 1 & 2 & \emptyset & 4 \\ [-low] \end{matrix}$$

It serves no special purpose to discuss these in detail, especially since Selkirk no longer adheres to them. Still, it is interesting to consider the metatheoretical background whence comes the remarkable garishness of these rules. I note that it is necessary to repeat three times (i) the syntactic conditioning, (ii) part of the phonological conditioning, and (iii) part of the structural change. In each rule we find (i) nearly identical stipulated boundaries and brackets, (ii) the factor ( C X ), and (iii) a deletion of a final consonant. (i) is necessary because the syntactic conditioning is encoded phonologically, in terms of terminal symbols, and because all three rules must be subject to it. This in turn is necessary because of (ii) and (iii). (ii) and (iii) follow because final nasal consonants and r's are not analyzed as L-consonants. They can't be, on a deletion theory, because of the ordering paradox which would arise. Suppose we allowed liaison (a) to remove final nasals and r's. Then there is no way to (non-globally) get (b) and (c) to apply in the right spots, i.e. if and only if (a) applied (or is going to apply, given the other order) to a final nasal consonant or r, respectively. Hence (a) must effectively be repeated in (b) and (c). None of these problems arises, given the rules in (52).

By the repeated factor      ( # C X ) # in (53) is meant the complement of (52)(a)'s      V, for these rules not only delete but apply non-vacuously in the non-elsewhere cases. The distinction

- (a) ... (  $\left\{ \begin{smallmatrix} + \\ \# \end{smallmatrix} \right\} C X ) \#$   
 (b) ... (  $(\#) C X ) \#$   
 (c) ... (  $\# C X ) \#$

is spurious, as far as I can tell. Apparent morpheme-internal nasalization (b) (dindon) is ruled out, even on a deletion analysis, by the alternation condition. On the insertion analysis, morpheme-internal de-nasalization can never happen because liaison can never insert a nasal consonant there. However, we do get alternations across  $\vdash$ : bonté, bonheur. As for the required  $\#$  in (c), it is based on examples like perdre, which does not get ER-converted to [pedrə]. On the insertion account, this shows exactly zero. None of our rules can do anything to /pɛdrə/. But even on the deletion account, the alternation condition rules out such a derivation. Finally, there are no examples (opposed to particularité, etc.) making it necessary to stop ER-Conversion across  $\vdash$ . On the insertion account, of course, particularité shows that application across  $\vdash$  is necessary.

My point is that, given the assumptions underlying them, (53)(a)-(c) should really appear as

- (a) ... (  $\left\{ \begin{smallmatrix} + \\ \# \end{smallmatrix} \right\} C X ) \#$   
 (b) ... (  $\left\{ \begin{smallmatrix} + \\ \# \end{smallmatrix} \right\} C X ) \#$   
 (c) ... (  $\left\{ \begin{smallmatrix} + \\ \# \end{smallmatrix} \right\} C X ) \#$

This result shows that the stipulated boundaries, which could in principle vary like any stipulated term, are in fact absolutely identical in the three rules. Note that the array

of boundary phenomena which (53)(a)-(c) happen to account for must follow given the treatment in (52)(a)-(c), which do not refer to boundaries at all and hence cannot repeat them. We do not have the option of playing with boundaries in our rules in order to get some other output.

Notice, too, that liaison as in (53)(a) is written so as to delete a consonant before another, word-final consonant, as well as deleting the final consonant before a word-initial consonant. Thus it is designed to collapse what have often been thought of as "truncation" and "final consonant deletion," respectively. The relevant facts are alternations like gran[t] homme, but grand[z] hommes. I've been restricting my attention to "final consonant deletion." But, as Klausenburger (1977) points out, on the insertion account there is no distinction in the first place. The L-consonant associated with grand can be inserted only before a vowel, not, for instance, before the plural morpheme Z. The L-consonant associated with Z is inserted under identical conditions.

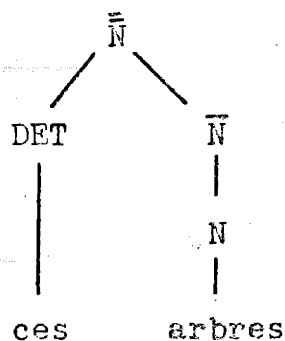
Klausenburger also observes that this treatment entails insertion of the L-consonant to mark the feminine. With respect to the le ~ l' alternation and the few other lexically-governed examples of final V alternating with zero, I see no clear implications for the necessary machinery.

I return finally to the matter of the residue left by (29). There are, as we know, words which are never found not making liaison before vowels. Determiners, for instance:

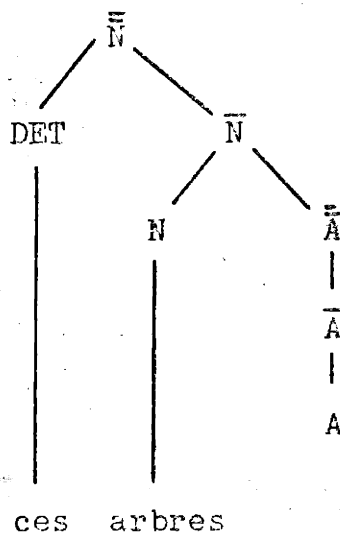
- (54) (a) ces^arbres  
 (b) ces^arbres énormes  
 (c) ces^espèces d'arbre  
 (d) ces^anciens arbres  
 (e) ces^assez gros arbres

I assume that corresponding to these strings are structures more or less like those in (54)'.

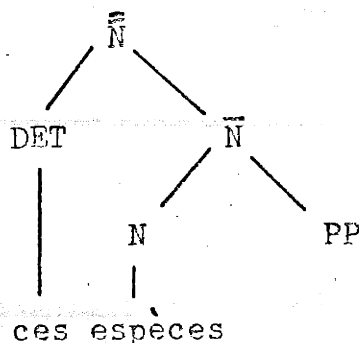
(54)' (a)

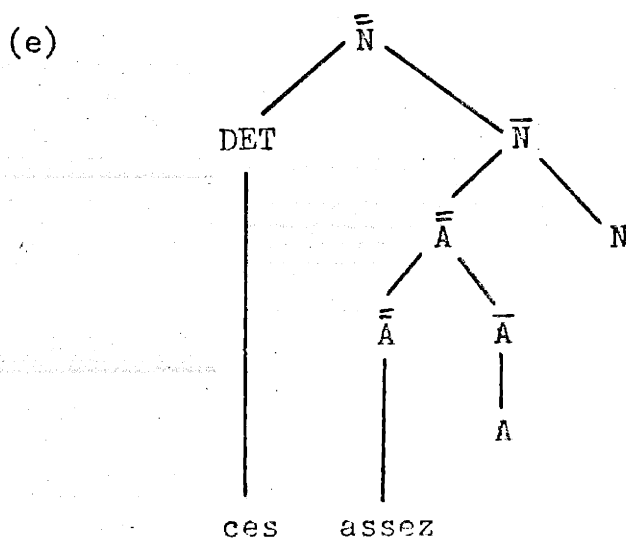
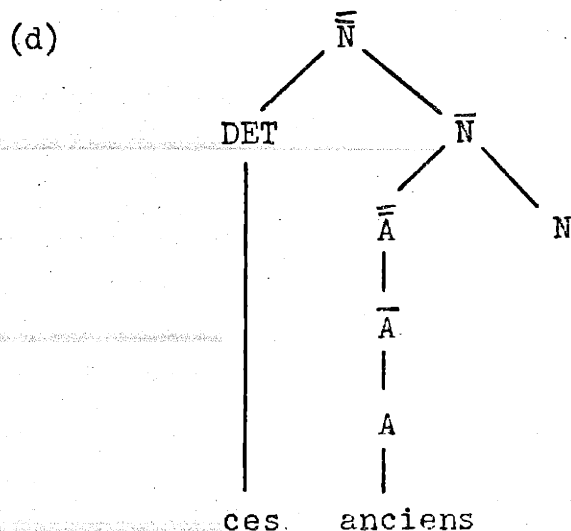


(b)



(c)





I see no reason to doubt that, at the point when liaison applies, determiners are syntactically just where the phrase structure rules put them, apart from the peculiar fact that they consistently make liaison. In all but the first example, of course, this conflicts with (29). It strikes me as an extraordinarily unattractive conclusion that *ces* must be re-adjoined more closely to the word immediately on the right: As far as I can make out, the only thing apparently making such an operation necessary is (29). Evidently, then, bearing the appropriate constituency relations is sufficient



for an item to be able to trigger liaison, but, for all non-lexical items, not necessary.

The class of unilateral liaison-makers includes specifiers of all kinds--articles (un, les, des, etc.), demonstratives (ces, cet), possessives (mon, ton, mes, tes), degree modifiers (très, bien, tout)--prepositions (en, chez, sans), and pronouns ("clitics")(en, les, nous).

With respect to this last case, pronouns, there is often supposed to be reason to believe that a pronoun defensively moved from somewhere else ends up being closely adjoined in some fashion to the next word. In French, for example, the sequence clitic-verb exhibits a list of interesting properties, all seeming to lead to the conclusion that the two are sisters under a node V: (i) Nothing can intervene between a clitic and a verb; (ii) a clitic cannot occur independently of a verb; (iii) a clitic cannot be modified; (iv) clitics occur in a strict order. (From Kayne 1975, pp. 81 ff.)

(55)(a) Jean toujours soupçonne Marie.

(b) \*Jean la toujours soupçonne.

(c) Jean la soupçonne toujours.

(56)(a) Qui est-ce qu'il soupçonne?

(b) \*La.

(c) Elle.

(57) \*Jean vous trois soupçonne.

(58)(a) Jean le lui donnera.

(b) \*Jean lui le donnera.

Thus we might like, with Kayne, to say that clitic and verb are co-constituent, both making up another V, which explains why the clitic behaves in the relevant respects like verbal morphology. Saying such a thing would, it is true, account for the fact that liaison between clitic and verb is obligatory.

(59) Jean  $\left\{ \begin{array}{l} \text{les} \\ \text{vous} \\ \text{nous} \end{array} \right\}$  abomine.

However, it seems to me that properties (i) through (iii) clearly follow from the existence of a late rule moving clitics to pre-verbal position. I confess I don't really see why property (iv), clitic ordering, says anything in particular about their relationship to the following verb.

After all, a noun phrase before a verb has internal ordering of constituents too, but that does not speak for its being proclitic. In any case, it is hardly overwhelmingly obvious that movable pronouns end up adjoined in any fancy way. I will assume, in fact, that there is no such adjunction operation, any more than in the cases of specifiers and prepositions.

The same four tests, incidentally, when applied to specifiers and prepositions, fail utterly, presumably underscoring the distinction moved/not moved rather than the distinction "cliticized"/not "cliticized": (i) There is no category that stands to a specifier or preposition as a verb stands to a clitic. In (54), for instance, the noun

is clearly not it, because various sorts of lexical material can comfortably intervene; (ii) specifiers and prepositions assuredly do not occur alone, but are like other non-lexical categories in this respect; (iii) specifiers and prepositions cannot be modified, but this, of course, is virtually the definition of non-lexicalness--categories which take modifiers do not lie outside the bar system; (iv) specifiers and prepositions occur only singly, hence obviating ordering problems.

The immediate point is that the special status of the non-lexical categories is essentially aconfigurational--it has nothing to do with their position with respect to the surrounding structure. It is simply true of them, in some way, that they make liaison. I take this opportunity to point out that, without boundaries, we also are powerless to define their exceptionality in terms of boundary weakness --i.e., fewer than two #'s.

I believe that this impotence is in fact appropriate. There are cases far more striking than (54). For instance, contraction in Modern Athenian Greek occurs to the right of a verbal clitic even though the verb is on the left.  
(From Kaisse 1977, pp. 125 ff.)

(60) *ðiàvasé to oposdípote* → ...t'oposdípote

read it anyway

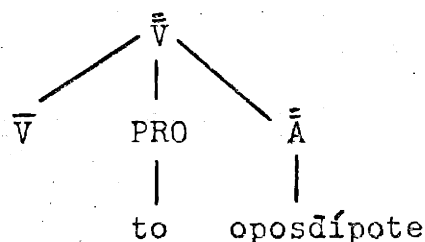
'Read it anyway.'

(61) *í mána tu éxi díó pedýá* → ...tu'xi

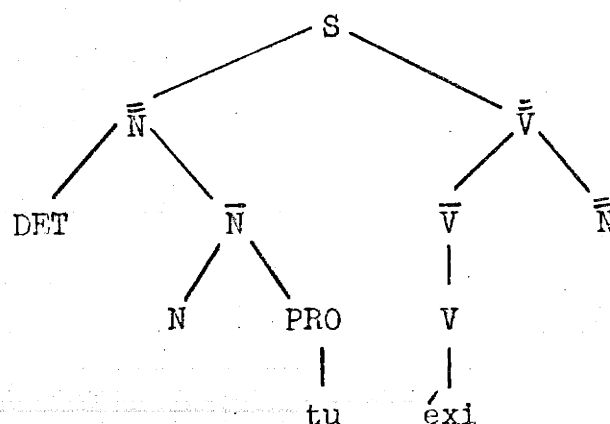
the mama his has two children

'His mama has two children.'

(60)'



(61)'



Contraction always happens between a clitic and a word to the right, supposing phonological conditions to be favorable. Note that no syntactic intimacy seems to be required. As Kaisse shows, pointing to the lack of word boundaries around clitics will not do either, since, in cases like (61), the N dominating the clitic will have its own #, ultimately leaving two between clitic and verb. Thus a contraction rule demanding the presence of a single word boundary will not be able to distinguish a sequence [N PRO] V, as in (61), from a sequence [N][V], where no contraction occurs.

Now since (29) concerns itself solely with constituent structure, defining non-lexicalness as (merely) a function of boundary weakness will be ineffective, at least if we want the non-lexical items to participate in the same rule of liaison. Examples like the preceding show that it is actually

wrong (in Greek, anyway). But, as there is no indication that we should syntactically re-adjoin them by special rule, we are left with the problem of how to represent their relationship to the next word.

Words usually considered to be clitics are notorious for behaving only partly as if they were affixes of the host word; if they behaved in every respect like affixes, there would be very little reason to think that they were not. On the other hand, they do not behave like completely autonomous words, either--witness the special phonology I've been talking about. Clearly we need some way of marking these special items as special. As I've suggested, it cannot be close phonological association (in terms of boundaries), and it cannot be close syntactic association.

Let us consider once again the French case. The fact, stated most baldly, is that liaison is always made by non-lexical categories. We clearly want to say in some way that making liaison goes along with belonging to a non-lexical category. The reader should be struck by the similarity of this as yet vague conclusion to the one reached with respect to the English and Irish rules of Parts I and II.

In all these cases, a rule always applies across a w-juncture if a non-lexical item is on the left.

I drive home this fact by noting that liaison, like the other examples, fails at high levels when w-juncture does not intervene--across trace, parenthesis, and outermost sentence edge.

(62)(a) Il y<sup>2</sup>en a beaucoup <sup>t</sup> / à rencontrer.

(b) Il y<sup>2</sup>a beaucoup à écrire.

(63) Il y<sup>2</sup>a beaucoup, / à ce qu'on dit, à écrire.

(64)(a) C'est nous. / Allons-y...

(b) Nous<sup>2</sup>allons.

(With respect to beaucoup, the appropriate category is not obvious, as with English lots. I'll just assume, with Selkirk (1972), that it belongs to a non-lexical category, called there Q.)

I think it's clear that we want to say in some fashion that a sequence of non-lexical item(s) plus lexical item  $[-N]_{-V}^*$ -w, separated by w-juncture(s), is a unit, call it a clitic group, intermediate between w and f. Although this last statement is not going to be universall true--it does not allow for enclisis--we will nevertheless be able to give a universal definition of the clitic group, henceforth abbreviated c. Having done this, we could reasonably omit the specification  $[-N]_{-V}$  in the left terms of rules like English monosyllabic destressing (Part I). We would simply say that they are c-level rules. Similarly, the Irish initial mutations are triggered by features belonging only to non-lexical items because these rules are morphologizations of older c-level rules.

Likewise liaison would be a c-level rule.

As for defining c properly, suppose we said that clitics are distinguished from other items merely in being marked [+P] or [+E], i.e. 'proclitic' or 'enclitic'. For

French, it appears that we can get away with saying that the non-lexical categories are all redundantly [+P].

$$(65) \begin{bmatrix} -N \\ -V \end{bmatrix} \rightarrow [+P]$$

Now, by a convention which is presumably universal,

(66) a clitic group  $c$  is a maximal sequence of terminal

$A_1 \dots A_n$  such that for every pair  $A_i A_{i+1}$

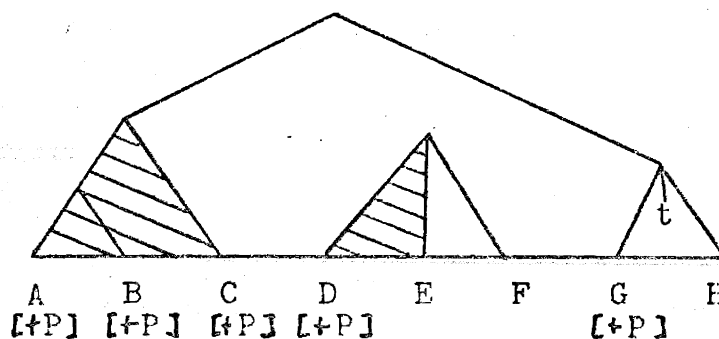
(i) there are nodes  $\alpha_1 \dots \alpha_n$  such that the figure

$\alpha_1 \dots \alpha_n A_i A_{i+1}$  bounds a w-juncture and

(ii)  $A_i$  is [+P] or  $A_{i+1}$  is [+E].

How this works might become clearer with an example.

(67)



A through H are words.  $\overline{ABC}$  and  $\overline{DE}$  are clitic groups, shaded in the figure. Since  $\overline{CD}$ ,  $\overline{FG}$ , and  $\overline{GH}$  are not sides of curves bounding any w-juncture and since E is not [+P], CD, EF, FG, and GH are not members of clitic groups. However, by the definition, F, G, and H, though words, are also clitic groups which happen to contain no clitics.

Thus homme, un homme, chez un de ces hommes are all clitic groups. Beaucoup/ t à rencontrer, beaucoup/à ce, nous/allons ((62)(a), (63), (64)(a)) are not clitic groups.

Now recall the conclusion we reached with respect to phrase-level liaison. We said that liaison applies before a right branch (29). Note that this formulation, accounting for f-level liaison, is totally distinct from what we just said about c-level liaison.

I believe that this distinction is exactly what's appropriate. It is a truly remarkable fact that it is precisely c-level liaison that is absolutely obligatory, and shows no sign of attrition (save via sporadic item-by-item loss of L-consonants), even in the most radical urban dialects. By contrast, f-level liaison has a growing optionality in conversational French, and is definitely on the way out.

In sum, liaison is an obligatory c-level rule, i.e. (68) is true of it,

$$(68) \quad W \left( \begin{smallmatrix} c \\ \text{ } \end{smallmatrix} X \text{ } w \text{ } w \text{ } Y \right) Z$$

and also an optional f-level rule under the familiar conditions.

$$(69) \quad X \left( \begin{smallmatrix} f \\ \text{ } \end{smallmatrix} Y \text{ } c \text{ } c \right) Z$$

(69) is shaky to varying degrees in modern dialects. Note that, having defined c, I now state (69) in terms of it. The reason is that there are contrasts like the following.

(70)(a) Il est<sup>^</sup>honnête.

(b) Il est<sup>^</sup>assez honnête.

(c) Il est/<sup>^</sup>évidemment honnête.

(71)(a) Il est<sup>^</sup>avocat.

(b) C'est<sup>^</sup>un avocat.



(c) Il est <sup>à</sup>avocat et législateur à la fois.

Assez honnête, un avocat are clitic groups. Évidemment honnête, avocat et législateur à la fois are not.

Why a sequence CONJ X, as in (23)(a), resists being a clitic group is a mystery which I have not succeeded in penetrating.

A concluding remark. It seems clear that every theory has to have some way of representing a word and its associated "clitics" as a unit. All a clitic is is a non-lexical item which behaves with respect to some grammatical process or processes (originally stress rules) as if it were included in a lexical item to its immediate left or right. What I've done, first, is to propose that there is such a unit, namely c, and that the processes usually thought to be diagnostic of an operation of cliticization are really rules at level c. Rather than performing some structural (syntactic or phonological) deformation to express cliticization, it is easier and less artificial to reduce clitichood to a lexical redundancy and to define c, in turn, in terms of it. In English, French, and Irish, the required redundancy rule is evidently very simple: All non-lexical items are [±P]. In languages with more complicated clitic systems, of course, we'll have to say special things about individual non-lexical items in the lexicon, or wherever it is that idiosyncratic information about non-lexical items goes.

## THE HEBREW EXAMPLE

As a second illustration I cite (though not at nearly as great length) the accentuation of the Hebrew Old Testament. I am indebted to John McCarthy for drawing my attention to Wickes (1881) and for a lot of help with the Hebrew.

There is a traditional system of diacritics accompanying the Old Testament, devised by unknown hands, which is designed to represent orthographically the details of the even more ancient declamation of the books. Actually there are several systems; I restrict my attention here to the one used in standard Tiberian Hebrew, since it is both the least degenerate in its details and the best documented. The cantillation was propagated orally until some time between the fifth and ninth centuries, when someone thought to actually write it into the texts, using a fantastically complex system of signs --the "accents." By means of these accents three aspects of the traditional Hebrew oratory are diacritically represented: the pronunciation, the music, and the intonation. (The vowel signs, also diacritic of pronunciation, were probably introduced at about the same time. See, however, Dotan (1964, 1972).) The accentuation takes the form of signs written above, below, before, or after (depending on the particular accent) the tonic syllable of each word, thus incidentally indicating primary stress. So much for the phonetic value of the accents. As for their original musical value, it is

almost totally lost. They probably stand for things like 'trill', 'sustain', etc. But--this is the interesting part--the intonational value turns out to be deducible from the texts themselves, together with a grammar of the language they're written in. I mean to say that there exists an algorithm for distributing the accents over any text. (This makes them--or at least the linguistic information borne by them, as we'll see--ultimately redundant, in much the same way as, for instance, question marks.)

It is very likely the case that the principal value of the accents is intonational, each accent mainly carrying one of two possible pieces of information: 'pause' or 'no pause'. As in many religious traditions, the melodic side of the scriptural reading is what is called "logogenic," i.e. more or less servile to and reflective of the syntactic and rhetorical organization of the text, though far from fully predictable from it. In fact the accentuation is known as pissuk te'amim, the division by the accents, evidently referring to their essentially pausal value.

Now my point is going to be that this same syntactic and rhetorical organization determines how the pauses are distributed, and in an interesting way. I don't claim to have discovered this interesting way; its discovery is largely the work of Christian accentuologists of the seventeenth and eighteenth centuries. See Florinus (1667), Spitzner (1786). More recent versions of the same idea are in Wickes (1881) and Spanier (1927).

The accents fall into two classes, the so-called disjunctive and conjunctive, having, along with an overlay of musical significance, the respective meanings 'pause follows' and 'pause does not follow'. (The absence of the one could not be allowed to stand for the other, because the melody still had to be notated.) We deduce that these pauses are more than merely part of a narrow religious tradition from the fact that they appear to condition authentic phonological processes. There is, for instance, a rule spirantizing initial stops after a word-final vowel. It applies only in the vowel-final word bears a conjunctive accent.

(70) wayyillāhamū bānē-yāhūdā  
and-waged-war sons-of-Judah  
'and the sons of Judah waged war' (Jud. 1 8)

(71) wayyis'ālu/bānē yis'rā'ēl  
and-asked sons-of Israel

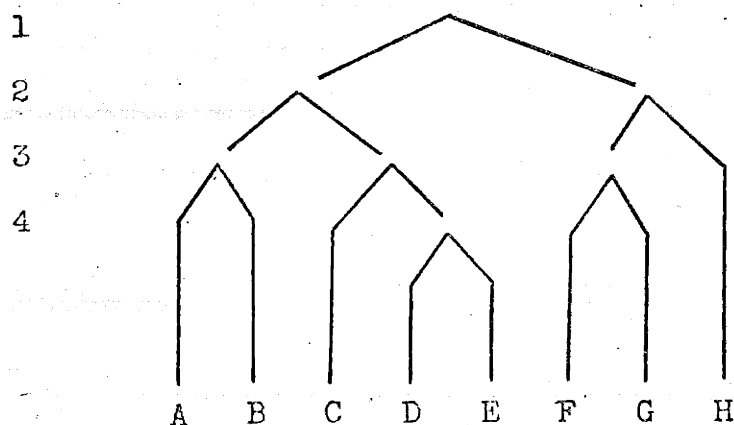
'and the sons of Israel asked' (Jud. 1 1)

(Spirantization is noted in the usual transliteration with the underline. I continue my notational practices in indicating conjunction with ', disjunction with /.)

There is an astonishingly involved set of rules (non-linguistic in nature) governing which accent out of one class or the other is actually used in the text. I will not discuss these. The system applied to the "Poetical Books" (Job, Psalms, and Proverbs) is slightly different from the one applied to the "Prose Books"; the reason is that a different,

more florid and artificial sort of melody accompanies the shorter verses of the Poetical Books. This matter I will not discuss either.

It seems that the Hebrew accentuators proceeded, either unconsciously or by way of conscious formalization of unconscious processes, as follows. (It's quite possible, incidentally, that they could have had some commerce with exponents of the quite active Hebrew grammatical tradition.) A binary branching is imposed on each verse, the highest branch being at its caesura. (The prose books are arbitrarily broken up into verse-length chunks for this purpose.) This process of unlabeled bracketing, called the "continuous dichotomy" by the Christian accentuologists, is repeated, top down, until no more three-word stretches are left unexamined.



Put differently, only two-word strings may stand undivided: AB, DE, and FG in the example. Words A, D, and F receive conjunctive accents. B, C, E, G, and H receive disjunctive accents. Which among the twenty-odd signs is actually found on

a word depends in part on which dichotomy (the numbers in the example) finally results in its marriage or separation--i.e. its distance, in number of nodes, from the root of the tree. But there is no reason to think that this orthographic distinction, with the aid of which an entire tree is reconstructible, was reflected in the spoken Hebrew by graded lengths of pause or no-pause. (See Wickes, II 14-15.)

This bracketing fairly clearly corresponds to the syntactic structure of the verse.

There are a few qualifications I must interject. The first is that the correspondence is fairly sloppy at gross levels of structure, because it is precisely here that binary syntactic branchings begin to become indefensible. I'm speaking of several kinds of lack of defense. One, the kind exhibited by adjacent sentences--separated by full stop--which often occupy a single verse and end up separated by the dichotomy; presumably no syntactic branching at all corresponds to this division. (Every word has to have an accent. There is no third possibility after conjunction and disjunction. In this last case, of course, the intonational value of the disjunctive, pause, is quite appropriate.) Two, the ternary branching of S immediately necessary for a VSO language such as Hebrew. Three, the workaday n-ary branchings made necessary at phrasal levels by multiple adverbs and PP's. In all these cases the (real, we're assuming) n-ary branching shows up in the texts as a nesting of n-1 fairly arbitrary binary ones. There are somewhat artificial rules for carry-

ing out this subordination, guaranteeing, in the main, left-branching structures.

The second qualification is that the dichotomy is easily bullied to one spot or another by the rhetorical purpose of the particular verse. Words which the accentuators (or the speakers they were transcribing) thought worthy of emphasis, even when they would otherwise (see below) receive conjunctive accents, show up set off by disjunctions. This of course is fairly natural on the assumption that a disjunction corresponds to a pause, in this case pregnant.

(72) hēn gôr yāgûr ?epes/nē?ôti

if strife one-strives not by-my-order

'If anyone stir up strife, it's not my fault.'

(Is. liv 15)

Similarly, though more amusingly, the accentuators were somewhat nervous about conjoining the name of the Lord to questionable objects. In such cases the holy titles often get one of the first disjunctions, even though the syntax may not happen to correspond.

Explainable exceptions aside, observe the results of the continuous dichotomy. Between any pair of words ultimately and exhaustively dominated by some node there is no pause. Between all other pairs of successive words some sort of pause is at least allowed.

The rule distributing no-pause at word edge is (73).

(73)  $\emptyset \rightarrow \text{no-pause} / \begin{cases} \emptyset \text{ —} \\ \text{—} \emptyset \end{cases}$

This we might collapse into (74), using mirror-image notation.

(74)  $\emptyset \rightarrow \text{no-pause} // \emptyset$

('Before or after a word, a pause is not allowed.' That this generalization is actually syllabic is a possibility which I have no way to investigate. Slightly unfortunately for this idea,  $C^{\wedge}C$  is found just as often as  $V^{\wedge}C$ .) (74), as written a word-level rule, has a condition on it, obvious enough from the way I've been describing the problem:

(75)  $X ( \underset{f}{w} w ) Y$

I'll give some examples of sole sisters held together by the accents and more distant relations separated by them.

A Hebrew conjunction is literally part of the word to its immediate right. There is evidently no phonological test enabling one to say that there is a word edge between the two. (I've been notating this synthetic arrangement with a hyphen in the gloss.) Therefore, unlike French and English, Hebrew cannot and does not practice Rossian subordination of the second conjunct, and it must be a sister of the first. In fact the two conjuncts consistently show up accentually glued together--

(76)  $w\acute{a}l\imath\check{s}\bar{o}l \quad bayy\hat{o}m \quad \frown \quad u\bar{b}a\check{l}l\bar{a}y\bar{l}\bar{a}(h)$

and-to-govern over-the-day and-over-the-night

(Gen. i 18)

(77)  $k\hat{i}-\bar{c}\bar{a}s\hat{i}\bar{t}\bar{a} \quad \quad \quad m\check{i}s\bar{p}\bar{a}\hat{t}\hat{i} \quad \frown \quad w\bar{a}\hat{d}\hat{i}\hat{n}\hat{i}$

for-you-have-performed my-judgment and-my-law

(Ps. ix 5)



(78) *yahalalûhû šamayim wāʔāreš*

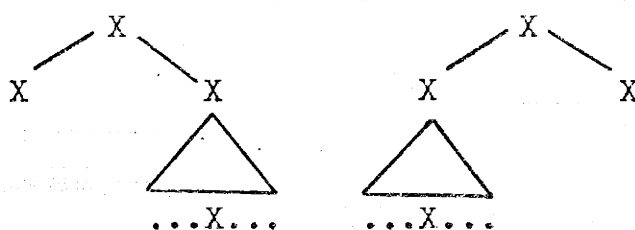
will-praise-him heavens and-the-earth (Ps. lxix 35)

--just as in the occasional asyndetic compound.

(79) *šinnîm pahîm*

arms (and-)swords (Prov. xxii 5)

However, as soon as either conjunct consists of more than a single word, the two zero-bar categories of the coordinate construction no longer exhaust it,



and a dichotomy is made.

(80) *yammîm/wəkol- rōmēs bām*

seas and-all crawling-things in-them (Ps. lxix 35)

(81) *yahweh šaddîq yibhan wərašāʕ/ wəʔohēb*

God righteous watches and-evil and-lover

*hāmās*

of-violence

'God watches the righteous and the evil and the lover of violence' (Ps. xi 5)

Conjoined verbs behave identically to the nouns in the foregoing examples:

(82) *ʔet-mî hērapṭā wagiddapṭā*

whom you-dreaded and-you-feared (Is. xxxvii 23)

(83) *šub qah-ləkā mēgillān ʔaheret*

do-again take scroll another

'take again another scroll' (Jer. xxxvii 23)

Again, a complex conjunct induces a disjunction:

- (84) yizkarû / wayāšubu ^ ?el-yahweh kol  
 will-remember and will-turn to-God all  
 ?apse-?āreṣ  
 ends-of-the-earth

'all the ends of the earth will remember and turn  
 to God' (Ps. xxii 27)

- (85) lō ^ yādāʿû / wālō ^ yābînû  
 they do-not-know and-they do-not-understand  
 (Ps. lxxxii 5)

Likewise conjoined adjectives, also sometimes asyndetic:

- (86) yahweh/ʿizzûzʿwəgibbôr  
 lord strong and-mighty (Ps. xxiv 8)
- (87) kî-yahweh/ʿelyônʿnōrā  
 lord high terrible (Ps. xlvii 3)

Nouns show up conjoined with all manner of single-word  
 complements--adjectives, objects of word-internal prepositions,  
 etc.

- (88) ?ēlʿgādol  
 God great (Ps. xcv 3)
- (89) wəšōhadʿal-nāqî  
 bribe against-the-innocent (Ps. xv 5)
- (90) šimhā(h)ʿlāʿîš  
 joy to-a-man (Prov. xv 23)

But when any qualifying expression is more than two words  
 long, a disjunctive accent falls on the preceding nominal.

Cf. (86), (87) and:

(91)  $kə^{c}ēs/sātūl$   $^{c}al-palgē māyim$   
 tree planted at-water-brooks (Ps. i 3)

(92)  $hā?ēl / hannōtēn$   $nāqāmōt$   $lî$   
 the-God the-one-giving vengeance to-me  
 'the God avenging me' (Ps. xviii 48)

(This last is a standard relative clause.)

An interesting case is the pseudo-appositive (very likely another example of asyndeton): King David, the-Lord God, etc., consistently conjoined by the accents. Compare the true appositive, just as consistently disjoined:

(93)  $?et-?oyābēkem / ?et-mō?āb$   
 ACC-your-enemies ACC-Moab  
 'your enemies, the Moabites' (Jud. iii 28)

(94)  $?et-barîti / šālôm$   
 ACC-my-covenant peace  
 'my covenant, peace' (Num. xxv 12)

(Note the intonational contrast even in the corresponding English.) Parentheticals, in general, have disjunctions on both sides:

(95)  $wayyišmā9$   $pašhur/ben-immēr$   $hakkōhēn/$   
 and-listened-to Pashur son-of-Immer the-priest  
 $wahû-pāqîd$   $nāqîd$   
 and-he-was-entrusted with-an-office  
 $bābēt yahweh /?et-yirmāyāhû$   
 in-the-temple ACC-Jeremiah

'Pashur, son of Immer the priest (and he was en-

trusted with an office in the temple) listened  
to Jeremiah<sup>1</sup> (Jer. xx 1)

Unfortunately, examples like this prove very little with  
respect to the possible failure of (74) across parenthesis,  
because we already know that a disjunction appears where  
any kind of pause occurs, even the kind that we can assume  
comes with Hebrew parentheticals. I also know of no signi-  
ficant examples of what one might assume is a trace blocking  
(74).

## THE ITALIAN EXAMPLE

As a final illustration of high-level phonology influenced by constituency relations among the words it involves, I'm going to briefly discuss a single rule of Italian, known as raddoppiamento (sometimes rafforzamento) sintattico 'syntactic doubling' ('strengthening'), which geminates an initial consonant under dialectally varying phonological conditions and under either of two pan-dialectal syntactic conditions. According to Napoli and Nespor (1977), "RS [the abbreviation I'll adopt from here on in] can apply between a word a and a following word b, where a is immediately dominated by the pre-terminal category symbol A and b by B,...if A is the left branch of the first node that dominates both A and B." This is the same as saying that RS applies between a and b if a begins a constituent to which b also belongs. In our terms, RS is subject to (96).

$$(96) \quad X \left( \begin{smallmatrix} \text{f} \\ \text{f} \end{smallmatrix} w w Y \right) Z$$

Also, "RS can apply on S between the first constituent and the second constituent..." That is,

$$(97) \quad \left( \begin{smallmatrix} \text{f} \\ \text{f} \end{smallmatrix} X w \right) w Y$$

For instance:

(98) Voglio una brocca d'acqua rossa.

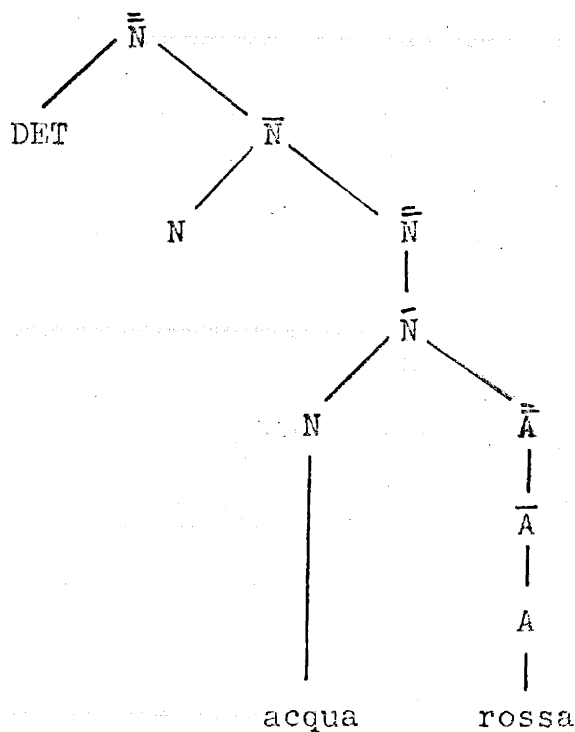
(a) 'I want a pitcher of red water.'

(b) 'I want a red pitcher of water.'

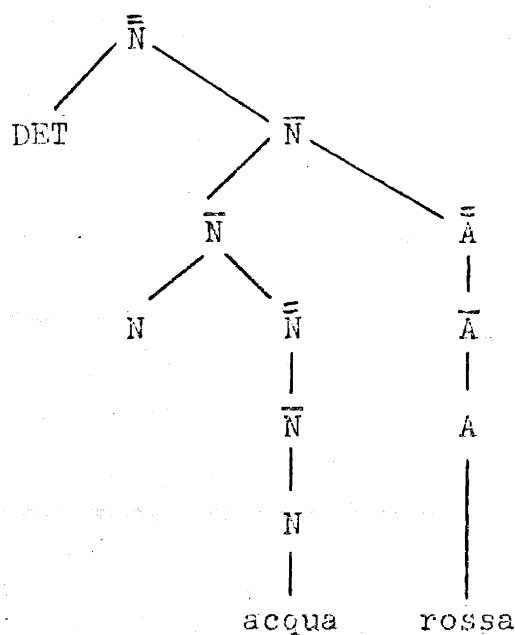
(Napoli and Nespor's examples throughout.) This sentence is

ambiguous. However, if RS doubles the initial of rossa, only the first reading turns out to be possible. That this should be so follows from (96).

(98) (a)'



(99) (b)'



Similarly, "old men and women" examples like (99).

(99) le ragazze e i ragazzi tedeschi

(a) 'the German [boys and girls]'

(b) 'the [German boys] and girls'

RS after ragazzi, by (96), forces the (b) interpretation.

In the next example,

(100) Maria verrà/sicuramente.

'Maria will come for sure.'

RS is impossible after verrà because sicuramente is a sentence adverb. Thus S branches ternarily, and verrà and sicuramente are the first words of no constituent. But suppose (100) lacked a subject noun phrase:

(101) Verrà<sup>^</sup>sicuramente.

'She'll come for sure.'

RS appears, by (96). (I do not take any position here on whether the subject in (101) went via a rule of subject pronoun drop or was simply absent to begin with. Recall that *w* is terminal, so that a dangling node will fail to be factored by either (96) or (97).)

Quite generally, in fact, in multiply branching structures  $[_{\alpha} a b c \dots n]$  ( $\alpha$  of arbitrary category and every  $k$  a word) the result is  $a \bar{b/c/\dots/n}$ .

(102)(a) È  $[_{\bar{A}} \text{ bagnato}^{\wedge} \text{di vino}]$ .

(b) È  $[_{\bar{A}} \text{ bagnato}^{\wedge} \text{di vino/da capo a piedi}]$ .

(c) È  $[_{\bar{A}} \text{ bagnato}^{\wedge} \text{da capo a piedi}]$ .

(a) 'He's wet with wine.'

(b) 'He's wet with wine from head to foot.'

(c) 'He's wet from head to foot.'

(103)(a) Ho visto tre  $L_{\bar{N}}$  grandi<sup>^</sup>cani.

(b) Ho visto tre  $L_{\bar{N}}$  grandi<sup>^</sup>cani/pastori.

(c) Ho visto tre  $L_{\bar{N}}$  cani<sup>^</sup>pastori.

(a) 'I saw three big dogs.'

(b) 'I saw three big German shepherds.'

(c) 'I saw three German shepherds.'

(104)(a) Ha  $L_{\bar{V}}$  già<sup>^</sup>parlato.

(b) Ha  $L_{\bar{V}}$  già<sup>^</sup>visto/Carla.

(c) Ha  $L_{\bar{V}}$  visto<sup>^</sup>Carla.

(a) 'He's already spoken.'

(b) 'He's already seen Carla.'

(c) 'He's seen Carla.'

(97) is responsible for RS in cases like (105)-(107).

(105) La religione che pratica<sup>^</sup>purtroppo perde fideli.

'The religion he practices unfortunately is losing  
followers.'

(106) Dopo martedì Maria non viene più.

'After Tuesday Maria isn't coming anymore.'

(107) Mangio i fagioli<sup>^</sup>Mario sono sicura.

'Mario ate the beans, I'm sure.'

I would like to point out that the existence of a phenomenon like the one accounted for by (97) poses a serious problem for any theory trying to do the same job by characterizing in some way the terminal symbols across which RS applies.

There is no arrangement of word boundaries which RS, governed by (97), will fail to breach. The reason is that it is not



the words on either side of the ligature in examples like (105)-(107) that must stand to each other in a stipulated structural relationship, but the phrases they belong to. In structures  $[_{\alpha} A B C \dots N]$  ( $\alpha = S$  and every  $K \in f$ ), the result, by (97), is  $A^{\wedge} B/C/\dots/N$ . So:

- (100)  $\bar{N}^{\wedge} \bar{V}/\bar{A}$  Maria verrà/sicuramente.  
 (108) (a)  $\bar{N}^{\wedge} \bar{A}/\bar{V}$  Maria<sup>^</sup>quando vorrà/verrà.  
 'Maria when she wants will come.'  
 (b)  $\bar{V}^{\wedge} \bar{N}/\bar{A}$  Verrà<sup>^</sup>Maria/di sicuro?  
 'Will Maria really come?'  
 (c)  $\bar{V}^{\wedge} \bar{A}/\bar{N}$  Verrano<sup>^</sup>di sicuro/quei fratelli?  
 'Will those brothers really come?'  
 (d)  $\bar{A}^{\wedge} \bar{N}/\bar{V}$  Sicuramente<sup>^</sup>Maria/verrà.  
 'Surely Maria will come.'  
 (e)  $\bar{A}^{\wedge} \bar{V}/\bar{N}$  Di sicuro<sup>^</sup>verranno/quei fratelli.  
 'Surely those brothers will come.'

I might note that (96) and (97) are not really as dissimilar as they appear. The import of both is that in  $[_{\alpha} A B C \dots N]$  only the left branch of  $\alpha$ , A, makes RS with what follows. If  $\alpha = S$ ,  $A \in f$ . If  $\alpha \in f$ ,  $A = w$ . So we might think of writing (109).

- (109)  $X (_{\alpha} \varepsilon \varepsilon Y) Z$ , where  $\alpha \in \{f, S\}$  and  $\varepsilon \in \{w, f\}$

It is interesting that there appears to be no evidence for c in Italian, at least not from RS. The reason is that Italian, like French, is right-nuclear. A specifier or preposition is never found as anything but A in  $[_{\alpha} A B C \dots]$ , which is always covered by (109).

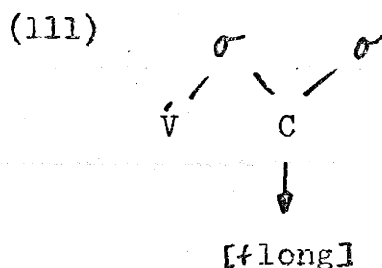
I ought to mention, finally, that RS appears to also have syllabic conditioning. It is essential to know, before going into this, just what the rule actually does. In the speech of Saltarelli (1970) and Vogel (1977), essentially "standard" Tuscan Italian, a lone initial consonant doubles following a stressed short vowel.

$$(110) \quad [+cons] \rightarrow [+long] / \begin{bmatrix} -cons \\ -long \\ +str \end{bmatrix} \text{ --- } V$$

Final vowels are always short in Italian. However, medial vowels may be long. Level theory has us predicting word-internal RS after ones that are not. In fact, it occurs. Compare, for instance, casa [ká:sa] 'house' and cassa [kás:a] 'box', which Saltarelli gets from /ka:sa/ and /kasa/ respectively. However, these pronunciations are obligatory, whereas RS at word edge is blocked by pause.

Now this kind of thing is the usual story for syllabic rules. Pause excludes a syllabic linkage. There are no pauses word-internally. Hence the facts, if we say that the operation of our rule is contingent upon a syllabic connection having been established. (Compare, for example, the case of English palatalization. We have obligatory pic[č]ture but optional pick[č]your. Presumably the same obligatory rule does both of these, but requires ambisyllabicity of the dental, which is optional at word edge. More on this in Part IV.) Vogel (1977) in fact justifies, to my mind totally convincingly, a rule of left capture of initial consonants for Italian.

She ends up saying, in essence, that only ambisyllabic consonants geminate. The rule becomes (111):



In this (in my opinion) peculiar situation, where (111) has both prosodic and non-prosodic structural conditioning, I suppose we ought to expect the diagnostics of the latter to defer to those of the former. Syllabic structure is built without any regard to the existing syntactic edifice, including w-juncture. (I refer the reader to the next Part for some justification of this statement.) Hence trace and parenthesis should not block RS.

This appears to be true. Napoli and Nespor cite examples like (112), for instance.

(112) Chi è che i bambini immaginano t grandi?

'Who is it that the children imagine big?'

It is especially interesting that RS is possible into a parenthetical, but not out of one.

(113) Non ne posso più sai/di Giorgio.

'I can't take any more, you know, of George.'

Note that it is still true that immaginano and più are left branches. Sai is not. So (109) is still appropriate. (1), incidentally, correctly allows [<sub>α</sub>... [parenthetical] ...] to be a <sub>α</sub>, as examples like (113) evidently require that

(109) permit, since, even given our conclusions (Part I) about the structural independence of the parenthetical, (1) is defined in terms of bracketing, rather than domination.

I'd like to make a concluding remark. It seems to me that the potential value of the theory of configurationally-conditioned sandhi which I developed and illustrated in this Part is great. I chose my examples in order to avoid problems of debatable constituent structure; the essentials of the structures which I assumed, without defense, were moderately clear for independent reasons. But there are, needless to say, many remaining problems in the area of derived syntactic structure and many constructions whose details or even gross features have not been worked out. Now given a theory of syntax-sensitive sandhi for some language, it should in principle be possible to learn about poorly-understood constituency relations from the observable phonological facts. Note that French, Italian, and Hebrew offer diagnostics for right branch, left branch, and sole sisterhood, respectively. The possibilities are enormous. Here, I think, is the theory's most useful and welcome application.

## PART IV

## ENGLISH PALATALIZATION

The information in a labeled bracketing is of two obvious sorts: The configurational, carried by the bracketing, and the categorial, carried by the labels. I've been preoccupied so far with configurational conditioning of high-level phonological and morphological rules--with rules which involve segments in adjacent words A and B provided A and B stand in some particular geometric relationship to one another, perhaps within some particular domain. The formalism I use does not, the reader will notice, allow me to stipulate that A or B be of some particular category. This is because I believe that there are no such rules.

In this Part I'm going to take a close look at a phonological rule of English which, to my knowledge, has never received one before. This is the rule of final dental palatalization, henceforth FDP. My purpose is first to illustrate a rule of high-level phonology which is totally obtuse to the configurational properties of lexical structure. Several subsidiary points will be made in the course of this illustration. It strikes me as immensely significant that FDP, as well as not being configuration-sensitive, contrasts in every major aspect of its behavior with what we've come to expect of rules which are. This fact, which I will not fail to demonstrate, supports the causal relationship I proposed be-

tween configuration-sensitivity and these diagnostics-- all of which follow because the w-juncture is as it were the medium of such rules. To the extent that a rule cannot be influenced by both lexical and prosodic structure (the Italian case (Part III) suggests that such an occurrence is not completely out of the question), FDP, not exhibiting w-juncture effects, could not be sensitive to constituency relations among lexical units.

FDP does exhibit a number of oddities connected with its syntactic conditioning. These at first glance make it look like some combination of categorial and configurational information must be available to the rule. Considered more carefully, however, it seems inescapable that all of FDP's apparent eccentricity is to be removed from the phonological rule itself and blamed on pragmatic factors. The conclusion is strong: There is actually no logical way for these factors to directly influence the phonology. Rather, what they must influence is the output of blind rules. FDP turns out to be a case something like the sociologically "variable rules" of Labov (1972), except that FDP, invariable, grinds out surface forms with variable social sanction. Whether it is necessary to think of the variability in Labov's examples as residing in the rules is an interesting question. I am ignorant of the relevant facts.

Let me begin by briefly discussing the phonological effects of FDP, first addressed in Selkirk (1972). FDP is a casual speech rule. This means that palatalization occurs

- (1) (a) could you [d] ~ [ʃ]  
 (b) can't you [t] ~ [č]  
 (c) as you [z] ~ [ž]  
 (d) unless you [s] ~ [š]  
 (e) can you [n] ~ [ň]

(2)  $\begin{bmatrix} \text{reor} \\ \text{+ant} \end{bmatrix} \rightarrow \begin{bmatrix} \text{+high} \\ \text{-ant} \end{bmatrix} / \text{---ly}$

(2) is definitely wrong. The left bracket after the focus bar, especially, has no theoretical status whatsoever. I want the reader to understand thereby only that the yod is supposed (by all investigators to date) to begin the next word. This is not, in point of fact, true, and the matter gets more attention below.

The yod itself generally lenites, often to the point of disappearing altogether, after triggering FDP. This occurrence varies somewhat from speaker to speaker, all other things, in particular the applicability of FDP, being equal. The weakening of the yod is an independent event, brought about by an independent rule. Observe, for instance, that it occurs even if the palatal preceding the weakened yod

was never a dental.

(3) Don't let them  $\left\{ \begin{array}{l} \text{(a) misjudge} \\ \text{(b) touch} \\ \text{(c) camouflage} \\ \text{(d) push} \end{array} \right\}$  you.

$$\left( \begin{array}{l} [\text{ɟ}] \\ [\text{c}^{\vee}] \\ [\text{ʒ}] \\ [\text{s}^{\vee}] \end{array} \right) \left( \begin{array}{l} [\text{y}^{\vee}\text{uw}] \\ [\text{uw}] \\ [\text{y}\text{ə}] \\ \text{?}[\text{ə}] \end{array} \right)$$

Consequently, in my examination of palatalization I will carefully distinguish this process, henceforth treated and referred to as y-deletion, from FDP. Also, starting with the last example, I will neglect to consider palatalization of [n]. My reason is that it is not easy to hear when it happens--evidently because it results in a segment still belonging to the same phoneme, something not true of the other four satisfyingly blatant changes. Clear-cut data will become essential later. In general, I will end up ignoring a great many equivocal facts associated with this basically fuzzy phenomenon.

There is a related rule, final dental assimilation, which for similar reasons I will mainly fail to consider. FDA assimilates a word-final dental in point of articulation to a word-initial palatal consonant.

(4)  $\left[ \begin{array}{l} +\text{cor} \\ +\text{ant} \end{array} \right] \rightarrow \left[ \begin{array}{l} +\text{high} \\ -\text{ant} \end{array} \right] / \text{---} \left[ \begin{array}{l} +\text{cons} \\ +\text{high} \\ -\text{ant} \end{array} \right]$



E.g.:

(5)(a)	a bad	joke	[jʃ]
	the worst		[tʃ]
	whose		?[ʒ]
	an even worse		?[sʃ]
(b)	a bad	choice	[tʃ]
	the worst		[tʃ]
	whose		?[ʒ]
	an even worse		?[sʃ]
(c)	bad	shape	[ʃ]
	the worst		[tʃ]
	whose		[ʒ]
	even worse		[sʃ]

(There are, of course, no word-initial [ʒ].) Already these facts are moderately murky. It is not so easy to distinguish dental from palatal underneath the following consonant, still less to decide which is grammatical. Even though one hopes that FDP and FDA are in fact special cases of a single rule, obtained by omitting the specification [+cons] in (4), I will mainly forget about the portion of its effects which is not really distinctly audible. The reader will soon come to appreciate this decision.

To return to (2), the next thing that should be observed is that stress, either before or after the consonant to be palatalized, does not prevent application of the rule.

- (6)(a) You couldn't, coul[ʃ] you?  
 You can, cán'[tʃ] you?

Did that surprí[<sup>ǃ</sup>z] you?

I see that doesn't impré[<sup>ǃ</sup>s] you.

- (b) coul[<sup>ǃ</sup>] yóu guys  
nobody bu[<sup>ǃ</sup>] yóu  
as smart a[<sup>ǃ</sup>] yóu  
unle[<sup>ǃ</sup>] yóu do

Though Cooper, Egido, and Paccia (1977) demonstrate that FDP is somewhat statistically less likely in examples like (7) than in examples like (8),

(7) John'll lend yóu his orgone accumulator.

(8) John'll lend yóu his orgone accumulator.

I want to drive home the fact, almost too obvious to mention, that intended emphasis goes hand in hand with clarity of enunciation, and that some slight hiatus tends to precede a contrastively or emphatically stressed initial syllable. (Recall the Hebrew example, in this connection.) Both of these will fight FDP. FDP obviously fails if obscurity of the palatalized dental and subsequently lost yod conflicts with the high information load of the emphasized word. Indeed, the essence of FDP's status as a "casual speech rule" is that its output is judged to be an obscuration--thus tolerated, for the most part, only in moments of high redundancy. Moments of high redundancy, in turn, are characteristic of informal social give-and-take. As for pause,

(9) \*No[<sup>ǃ</sup>]...you!

FDP's two terms must evidently be on the same side of it.

My point is that the stress effect, though real, is not the

result of some stipulation in (2).

Now why should it be true that FDP, in contrast to the other rules I've discussed, fails across pause? The answer, I believe, is that FDP has direct syllabic conditioning. Kahn (1976), addressing himself to the problem of syllabic linkages established across word edge, proposes (10) below (his Rule V).

(10) In C V , associate C and  $\sigma$ .

|  
σ

This C, unassociated by Kahn's other rules of syllabification (see Part I above), must be a word-final one. Kahn notes that (10) applies only in "connected," i.e. pauseless, speech. It is precisely in pauseless speech that FDP applies, followed by  $\gamma$ -deletion. Put another way, wherever FDP or  $\gamma$ -deletion has applied, (10) has also:

(11)(a) I'll ge[ $\check{c}$ ] (\*...) [yuw] later. (FDP)

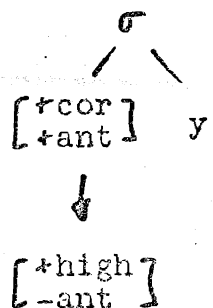
(b) I'll ge[ $\check{c}$ ] (\*...) [uw] later. (FDP >  $\gamma$ -deletion)

(c) I'll cat[ $\check{c}$ ] (\*...) [uw] later ( $\gamma$ -deletion)

I infer that both rules are in fact conditioned by the tautosyllabicity of the final consonant and the following vowel. Failure of either one across any sort of pause is a consequence. I am assuming, of course, that "V" in Kahn's formulation includes a possible [y] on-glide. It plainly must: Compare the f's of few, nephew, if you.

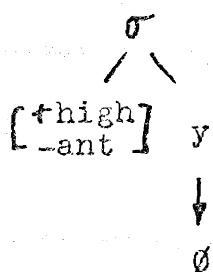
The new version of FDP is as in (12) on the next page.

(12)



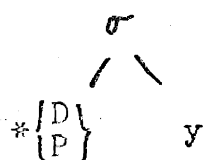
y-deletion we can write as (13).

(13)



These two rules, which we can make obligatory, but conditioned by the optional (10), conspire to rid the language of all final dentals and palatals

(14)



before tautosyllabic yod.

The next point to establish is that the word whose final dental suffers FDP may belong to any category.

- (15) N    the nigh[č̣] you left  
 A    how ridiculou[ṣ] you are  
 V    I misle[j̣] you  
 ADV   but instea[j̣] you  
 AUX   coul[j̣] you  
 P    besi[j̣] you

CONJ an[<sup>✓</sup>] you

COMP tha[<sup>✓</sup>] you

PRO tho[<sup>✓</sup>] you respect

It may stand in any syntactic relation to the following [y]-initial word.

(16) (a) John like[<sup>✓</sup>] you.

(b) John like[<sup>✓</sup>] you guys.

(c) John like[<sup>✓</sup>] you and your dentist.

(17) (a) John really like[<sup>✓</sup>] you.

(b) John liked and respecte[<sup>✓</sup>] you.

(c) John really liked and respecte[<sup>✓</sup>] you.

(18) John really liked and respecte[<sup>✓</sup>] you and your  
dentist.

(19) (a) the god[<sup>✓</sup>] you worship

(b) the minor pagan god[<sup>✓</sup>] you worship

(c) the minor pagan gods and goddesse[<sup>✓</sup>] you worship

(20) the minor pagan gods and goddesse[<sup>✓</sup>] you and  
your dentist worship

(21) (a) John said tha[<sup>✓</sup>] you'd be here.

(b) John sai[<sup>✓</sup>] you'd be here.

(22) (a) ?Whatever he sai[<sup>✓</sup>] you laughed uproariously at.

(b) ?After that outburs[<sup>✓</sup>] you sure looked shocked.

(c) ?He's so sozzle[<sup>✓</sup>] you'd better take him home.

In particular, the complexity of the constituents containing either of FDP's two terms varies independently of FDP's applicability, as does their syntactic distance, measured (say) in number of intervening brackets. Examples like (18) effec-

tively dampen the notion, tentatively conceived in Selkirk (1972), that more than one word boundary blocks FDP:

(18)(a) John [[#liked#]Iyou]]

(b) John [[#liked#]I#you guys#]]

(c) John [[#liked#]I#you and your dentist#]]

FDP once again becomes slightly questionable (22) precisely where the absence of a pause drops in acceptability.

Let us now ask ourselves what sorts of [y]-initial words may follow the palatalized consonant, as it turns out a more interesting question. First, look at (23).

(23)(a) all excep[<sup>✓</sup>c] you

(b) all excep[<sup>✓</sup>c] yourself

(c) all excep[<sup>✓</sup>c] your brother

(d) ?all excep[<sup>✓</sup>c] yours

(23)(d) is mysteriously inferior for some speakers. I have nothing very strong to say about this at the moment. Generally, FDP before a pronoun seems moderately successful. As for the other categories, most of them do not fare nearly as well.

I find that my speech allows the following:

(24) PRO Don't ki[<sup>✓</sup>j] yourself.

Jus[<sup>✓</sup>c] you wait.

Who'[<sup>✓</sup>ʒ] your shrink?

What'[<sup>✓</sup>s] your number?

COMP (no examples)

CONJ (no examples)

P (no examples)

AUX (no examples)

ADV I just got can[<sup>✓</sup>j] yesterday.

Aren't you finish[<sup>✓</sup>č] yet?

I did tho[<sup>✓</sup>ž] yesterday.

That'[<sup>✓</sup>š] yet another problem.

V I'[<sup>✓</sup>j] use another example.

?Jus[<sup>✓</sup>č] use your head.

?\*John'[<sup>✓</sup>ž] yodeling upstairs.

?\*Thi[<sup>✓</sup>š] yields two dozen, it says.

A \*Today's my dreade[<sup>✓</sup>j] yearly checkup.

\*I'm not tha[<sup>✓</sup>č] young anymore.

\*These are from John'[<sup>✓</sup>ž] yellow period.

\*Thi[<sup>✓</sup>š] urinary infection's getting me down.

N \*He's my thir[<sup>✓</sup>j] urologist in as many months.

\*It's jus[<sup>✓</sup>č] yogurt, it won't kill you.

\*John'[<sup>✓</sup>ž] Yiddish is really appalling.

\*How much is thi[<sup>✓</sup>š] ukelele?

There are speakers who dislike V, as much as A or N, following an FDP-derived consonant, or accept any category but N.

(There are no complementizers, conjunctions, prepositions, or auxiliaries beginning in [y].) The first generalization we might make is that the more categorial features a word shares with nouns, the more difficult it becomes to palatalize a preceding dental. It is clearly the category of the [y]-initial word that matters, and not that of any phrase it begins. (18)(a-c) are identical with respect to FDP.

A first description of the facts is the nouniness squish in (26).

(26) In D1  $[_{\alpha}y]$ , D a dental to be palatalized,

$$\alpha = \{N < A < V < \text{everything else}\}$$

("<" means "less grammatically than." Note that we cannot say simply that  $\alpha$  must be a non-lexical category: Verbs are often permissible. It seems to me that contrasts like (27) (a-d), for instance, are quite real.

(27) (a) My brother $[z^{\vee}]$  use Breem regularly.

(b) I noticed my brother $[z^{\vee}]$  using Breem.

(c) \*I noticed my brother $[z^{\vee}]$ ' used Breem.

(d) \*My brother $[z^{\vee}]$ ' use of Breem is getting compulsive.

(26) must be wrong. I'd like to nip in the bud the idea that it or anything like it is directly conditioning FDP. I think on the contrary that word frequency, not category membership, is the true conditioning factor: The more frequently occurring a word is--the more characteristic of normal conversation--the better it sounds following a palatal from FDP. This effect should follow in an obvious way from FDP's status as a casual speech rule, though it is still curious that the frequency of the word actually affected by FDP is irrelevant.

Frequency (currency in everyday conversation) gives roughly the same ranking of categories as (26). The roughness, as we'll see, is crucial. It is of course true that in normal speech one finds a small number of frequently-occurring function words, a larger number of lexical predicates, and quite a considerable number of relatively rarely used lexical arguments to put in them. I find speaking of word frequency more attractive and likely to be accurate than speaking of nouni-



ness for several reasons. The first is that there are a good many perturbations in the gross ranking in (26), most of them traceable to clear frequency effects. On the nouniness account, though, they are exceptional. For instance, FDP before verbs, not marvelous to begin with, is definitely enfeebled as the verb becomes exotic.

- (28)(a) I' [j] use
- (b) ?I' [j] utilize
- (c) ?\*I' [j] euphuize

Yon and yonder, infrequent to the point of nonoccurrence in most people's speech, sound very bad after FDP, though they presumably belong to non-lexical categories.

- (29) \*hither an [j] yon
- (30) \*that For [j] yonder

Universal, usually alone among adjectives, allows FDP to a certain extent in the speech of many linguists, among whom it has exceptional currency.

- (31)(a) tho [z] universal rules

Compare, on the one hand,

- (b) \*that rule' [z] universality

and

- (c) \*thor [z] unicellular organisms

on the other, neither one of which is at all good. One presumes that (c) would be in, and (a) out, for a microbiologist, for instance.

It is also interesting that adverbs, when de-adjectival or de-nominal, are no better in the eyes of FDP than the ad-

jectives or nouns inside: youthfully, usefully, yellowly, usually. (Compare the underived yet, yesterday.) This lack of improvement seems to show that category is not a factor. Similarly the verb yo-yo, de-nominal, is treated by FDP just like the noun, as (26) leads us not to expect.

(32)(a) \*tho[<sup>✓</sup>z] yo-yos

(b) \*tho[<sup>✓</sup>z] yo-yo like crazy if you're not careful

The one noun which does not fairly unequivocally block FDP is year: thi[<sup>✓</sup>s] year, las[<sup>✓</sup>c] year, nex[<sup>✓</sup>c] year, firs[<sup>✓</sup>c]-year, secon[<sup>✓</sup>j]-year, thir[<sup>✓</sup>j]-year. Not coincidentally, year leads by far all other [y]-initial nouns in frequency of occurrence. Alone among them, in fact, it regularly appears in compounds, at least some of which are surely lexical, like the preceding six. Year also permits FDP, though slightly less naturally, when not part of a compound.

(33)(a) my firs[<sup>✓</sup>c] year here

(b) my firs[<sup>✓</sup>c]-year students

(33)(a) seems marginally worse than (b), though not bad at all. FDP is quite productive before year:

(34)(a) twenty-eigh[<sup>✓</sup>c] years ago

(b) those waste[<sup>✓</sup>j] years

(c) the wors[<sup>✓</sup>c] year by far

It cannot be that anything about the phonological makeup of year is distinguishing it: Compare yeast and yield, for example. Thi[<sup>✓</sup>s] yeast, thi[<sup>✓</sup>s] yield seem impossible to my ear.

I ought to establish, more generally, that the phonologi-

cal properties of the word of category  $\alpha$  are irrelevant to the applicability of FDP. Let us first consider the initial vowel nuclei found in words successfully following an FDP-derived initial.

(35)				úw, uw
	é, e		ə	ów
			ʌ	

Examples:

(35)'		yóu, you
	yesterday, yet; yourself; yours	
	ya	

Conversely, the initial nuclei of the unsuccessful words:

(36)	í	íy		úw, uw
	é, e	éy	ə	ó
			ʌ, ʌ	ów
	æ			á

As:

(36)'	Yiddish, yeast;	euphony, euphonic
	yellow, Yehudi; Yale; Uranus; yod; yo-yo	
	yucca, Yahoo	
	yam	yacht

Every initial nucleus (35) offered by the poor collection of [y]-initial words in (35)' is also found among the ones in (36). Notice also that number of syllables is irrelevant, as is stress: Recall (7), on the one hand, and compare \*bu[č] euphony, \*qui[č] euphonic on the other.

The theory, then, says so far that FDP becomes more dif-

ficult as the following [y]-initial word drops in frequency. This kind of thing is, of course, the usual story with casual speech rules. Observe, for instance, the possibility, progressively smaller top to bottom, of reducing the final [ow] in the following words,

- (37) tomato
- potato
- borrow
- wheelbarrow
- shadow
- sorrow
- kimono
- Plato

or, a slightly weaker effect, of reducing to, have, after various verbs.

- (38) seem to
- try to
- attempt to
- strive to
- endeavor to
- (39) could have
- might have
- can have
- will have
- shall have

In all four cases the possibility of performing the rule varies directly as frequency of locution. (This type of

phenomenon is extensively documented in Fidelholtz (1975).)

All of this is moderately understandable. There are two reasons, as far as I can tell, for effects like these. First, the more removed from the vernacular a word is, the more fastidiously it will tend to be pronounced, since its information load is correspondingly greater. Second, a style of speech going in for arcane vocabulary is not generally a style to tend to allow reduction by casual speech rules in the first place; these two styles will obviously tend to exclude one another. Reasons one and two are not the same: Compare (37) with (38) and (39).

Conversely, frequent usage can drive a word into the purview of a rule which otherwise excludes it. The classic case (due to Fidelholtz) is the anomalously frequent reduction of bassoon to [bəsún] in the speech of bassoonists. Nontense weakly-stressed vowels normally resist reduction in closed syllables  $C_0\_C_2$  (SPE, p. 121): Compare monstrosity, demonstrate; sensation, insensate. (To close the initial syllable of bassoon [bəsún] we probably have to supply an underlying geminate s.) This is obviously very like the universal example above.

Note now the following interesting contrast.

(40) Bu[č̌] you have three lemons.

(41) \*Bu[č̌] "you" has three letters.

I invite the reader to control for stress and pause.

A quotation is a noun. This is evident enough from the fact that it takes nominal determiners and complements.

(42) [<sub>N</sub> [<sub>DET</sub> That] [<sub>N</sub> [<sub>N</sub> "you"] [<sub>PP</sub> in the last line]]]

is just about illegible.

Note also that "you", in addition to taking modifiers, governs agreement like a singular noun, not like a second-person pronoun, and will of course continue to do so regardless of the contents of the inverted commas.

(43)(a) \*But "you" have three letters.

(b) \*That "you" are illegible.

A quotation can obviously be anything at all--ungrammatical English, grammatical strings in other languages, ungrammatical strings in other languages, noises significant in no language--without affecting the grammaticality of the outer English sentence.

(44) "Q" is not English.

A grammar, as I mentioned in Part I, cannot be held responsible for specifying Q; it is an absolutely arbitrary vocalization. In (43) it happens to correspond to an English word, [<sub>PRO</sub>you]. My point is that "you" stands to you as, say, "oui" stands to we: "you" is not structurally a pronoun. At the very least, there is no test according to which it does not behave only as a noun. If "you" were a pronoun, (45) would be bad.

(45) "You is" shows faulty agreement.

I mean to suggest that "you" is to be represented as [<sub>N</sub>you], not [<sub>N</sub> [<sub>PRO</sub>you]].

Therefore we may say that a quotation is a word--i.e. something dominated by a terminal category--of frequency so

low that it is not even listed in the lexicon. Indeed, it can't be, since it's arbitrary. This explains why FDP fails before "you", just as before any other [y]-initial but linguistically insignificant noise.

I want to quickly destroy the idea that some sort of quotation effect insulates what's inside the quotes phonologically in cases like (41). Pause, first of all, is clearly not the culprit, since a pauseless rendering of (41) improves matters for FDP not one bit. Second, note that it is the frequency only of the [y]-initial word that affects FDP. Quotations, like all [y]-initial nouns but year, block it. The frequency of the dental-final word is irrelevant. One imagines, then, that a quotation should allow its final dental to palatalize. So it is:

(46) That's the last "plea<sup>✓</sup>[z̥]" you'll have to say.

Note also:

(47)(a) Have you read Kidnapp<sup>✓</sup>[ç] yet?

(b) Have you been kidnapp<sup>✓</sup>[ç] yet?

(48)(a) I read "The Frog<sup>✓</sup>[z̥]" yesterday.

(b) I dissected the frog<sup>✓</sup>[z̥] yesterday.

By contrast:

(49)(a) \*Bu<sup>✓</sup>[ç] You Can't Go Home Again is fairly self-indulgent writing, to my mind.

(b) Bu<sup>✓</sup>[ç] you can't go home again.

As I mentioned in Part I, there are dialects of Australian English which palatalize everywhere a dental and a yod pauselessly meet, totally regardless of any non-phono-

logical information, in particular about word frequency. In Australia, then, FDP is not a casual speech rule: tha[č] yo-yo, John know[z] Yerkish, qui[č] yelling, etc. We leap to predict that, for speakers of these dialects, examples like (41) should go through with FDP, since the fact that a quotation is a low-frequency noun will be true but irrelevant. This is actually the case. (Thanks to David Nash.)

The theory that it is FDP that is affected by word frequency is, however, incorrect. It seems to me slightly bizarre to begin with that only the frequency of the [y]-initial word should be of relevance. Notice also that FDA (4) is not at all sensitive to word frequency, as (5) shows. Therefore we will not be successful in collapsing FDA and FDP, even though this result is of course desirable.

In what respect to FDA and FDP differ? Y-deletion applies obligatorily to the output of FDP, but not to the output of FDA. Investigating one step further, it turns out that y-deletion alone is bad exactly where FDP followed obligatorily by y-deletion is.

(50)(a) \*I taugh[čɪ]ddish nights.

(b) \*I tea[čɪ]ddish nights.

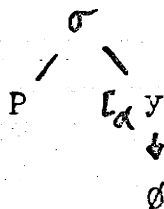
(51)(a) \*Tha[čow]deling is against the regulations.

(b) \*Su[čow]deling is against the regulations.

Evidently y-deletion is the rule subject to our frequency effect. Nevertheless, it must be wrong to write y-deletion as (52),



(52)



with some condition on the frequency of word  $\alpha$ , because the result of not applying  $\gamma$ -deletion is also bad when and only when FDP has already applied.

(53) \*I taugh[čy]iddish nights.

(54) I tea[čy]iddish nights.

In (54), obviously, Kahn's Rule V ((10) above) has not applied, and therefore  $\gamma$ -deletion has no chance to. But in (53), (10) must have applied, since FDP has. It appears that  $\gamma$ -deletion must remain absolutely obligatory.

The responsibility for the phenomenon must, I think, be pinned on an output condition, sociolinguistic in nature: The less frequent a word--the less characteristic of ordinary conversation--the more stigmatized its derivation when it includes the loss of its initial segment. This simple idea explains every fact we've considered.

$\gamma$ -deletion is virtually the only rule in English which deletes initial segments. A second example which comes to mind,  $h$ -deletion before unstressed vowels, is also constrained by word frequency. Compare:

(55) herself

historical

hysterical

heraldic

Hieronymus

hendiadys

The only other example I can think of in English is the somewhat childish clipping of initial unstressed non-tense vowels which one sometimes hears: 'parently, 'merican. This too is subject to a frequency effect. Clipping seems progressively more difficult in the words in (56).

- (56) apartment
- effectively
- abusive
- epenthesis
- effulgence
- agouti

(On this account, incidentally, we can see why an urologist (see Part I) is going to be bad for palatalizers. Since the n of the article must be ambisyllabic (recall (25)), it palatalizes and the y drops. But this is not allowed for words like urologist. And unlike this urologist, an urologist cannot simply surface without the syllabic connection having been established. The only option is not to have inserted the n in the first place. There is no English word in [yuw-] which is both frequent enough to allow y-deletion and possible after the article.)

Nevertheless, it is surely a mistake to think, as does Allen (1962) that high information load at word onset universally militates against rule-governed violence to initial segments. Allen's (pp. 17 ff.) underlying assumption, that

intelligibility of output cannot be reduced by rule, is plainly unjustified: Any rule which takes A to B in some environment is giving the hearer an additional task in recovering A. (Surely commonplace reductions like [čə'dʌn] 'what are you doing' are not performed in the interests of intelligibility.) The prediction, too, is false, as attested, for instance, by the Celtic case (above) and the Awjtim case (Hale 1976), in which initials are regularly and massively affected. No, the explanation for the weakness of word-initial deletion in English lies in its relation to the rest of standard English phonology--these are the only rules affecting initials, and not one is at all well-entrenched. Hence the opacity of derivations in which they apply, unless aided by informational redundancy. Note too that none of the three is restricted to word-initial application. We also have h-deletion, elision, and palatalization medially: prohibition, potato, textual.

This last assertion needs further commentary. We have successfully removed all conditions but the syllabic one from FDP. (12), taken at face value, leads us to expect palatalization word-internally. And in fact there is a well-known rule of palatalization (SPE, p. 230) doing just this.

$$(57) \begin{bmatrix} -\text{son} \\ +\text{cor} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{ant} \\ +\text{strid} \end{bmatrix} / \text{---} \begin{bmatrix} -\text{cons} \\ -\text{voc} \\ -\text{back} \end{bmatrix} \begin{bmatrix} -\text{cons} \\ -\text{stress} \end{bmatrix}$$

(57) is the rule responsible for node~nodule, fact~factual, seize~seizure, sex~sexual. It is part of the word-level phonology, because in SPE theory an optional word boundary must

appear in structural descriptions as (#). Apart from this, (57) is virtually identical to FDP sans syllabic conditioning. And the syllabic conditioning, I observe, is always met word-internally. The only thing stopping us from saying that (57) is just a special case of (12) is the stipulation in (57) to the effect that the vowel following the yod must be unstressed.

However, this stipulation is wrong. It is based on examples like ensue, resume, perpetuity ([-súw, -zúwm, -túwrtiy]), which I really see no reason to give underlying /y/, fated to delete, at all. What we need is a good example of an independently justifiable [XD] clearly alternating with [XP] yV. Such examples are nearly nonexistent, but I offer, along with grains of salt, credulity [krɛjúwɫitiy] (for many speakers), the persuasiveness of which depends on its derivation from /kred/ (cf. credibility, etc.); and individity [ɪndəvɪjúwrtiy] (divide).

Actually, examples of real words are slightly irrelevant at this point. A small gedankenexperiment will tell us all we need to know. Imagine a word zeft, derived from which there is an adjective in -urious. The result is clearly [zɛfɔ́riyas], not [zɛft́riyas]. Or imagine a word gand, also appearing inside a noun in -ulity: [gɔ́nɟúwɫitiy], not [gɔ́ndúwɫitiy].

The point I am making is this. One repeatedly finds high-level phonological rules which are merely less punchy versions of independently necessary word-level ones. (See, e.g., Harris (1968), Cheng (1973), Rudes (1976), Bolozky (1977).) The wider rule does the same thing as the narrower

one, but applies with nothing like the same regularity, and only in connected speech. In every such case, I predict, there is in fact only one rule, an utterance-level one sensitive to prosodic structure. Since prosodic structure changes with faster speech, generalizing itself over progressively larger domains, so will the applicability of the rule. There may also be various types of social sanctions against its application: Hence "casual speech rules," which are often also fast speech rules.

Finally, I want to assemble several significant facts about FDP, now more accurately referred to as DP. DP is an utterance-level rule whose two terms often lie on either side of a w-juncture. However, unlike all the rules of Part I,

i) DP is blocked by pause.

ii) DP ignores phrase structure, as long as it is not reflected by pause. It is false, in particular, that DP is clause-bound, as suggested by Postal (1974). His examples, like (58) and (59),

(58) \*I met Mary, who is sitting on the ma[č̣], yesterday.

(59) \*Shaking the po[č̣] yields few benefits.

disallow DP (or y-deletion) for independent reasons--namely the presence of pause (58) and of a low-frequency verb (59) to the right of the palatalized consonant. Compare (60) and (61), which have no subordinate clauses.

(60) \*I met Mary, your bo[ṣ̌], yesterday.

(61) \*Tha[č̣] yields few benefits.

On the other hand, note (62), (63), and (64), which do:

(62) I gue[ṣ̌] you'll be glad to hear I got a job.

(63) This prov[<sup>✓</sup>z] you're mistaken.

(64) They never got the letter[<sup>✓</sup>z] you sai[<sup>✓</sup>j] you sent.

iii) DP is not blocked by parenthesis, to the extent that pause does not accompany it.

(65) I coul[<sup>✓</sup>j] (,) y'see, but I won't.

(66) I had thi[<sup>✓</sup>s] (,) y'know (,) like really insane idea.

(67) Quie[<sup>✓</sup>l] (,) you guys, I'm trying to sleep.

Conversely, when the interpolated material ends in a dental:

(68) Quiet, you guy[<sup>✓</sup>z] (,) you're driving me bananas.

(69) "Come on," he say[<sup>✓</sup>z] (,) "you're putting me on."

(70) This is, I gue[<sup>✓</sup>s] (,) your last chance.

iv) DP is not blocked by trace, again to the extent that pause does not accompany it.

(71) I didn't like the apartment I looked a[<sup>✓</sup>c] t yester-day.

(72) I can't understand what it i[<sup>✓</sup>z] t you're saying.

(73) I said I woul[<sup>✓</sup>j] t (,) your honor.

(74) John says he's your friend more than he i[<sup>✓</sup>z] t your lover.

(As in Part I, I do not necessarily assume that all of these are derived by movement. A "trace," in my usage here, is the pre-terminal structural residue of any extraction. In each case, we know it is there because the non-lexical item at, is, would, is preceding the gap is not allowed to distress or reduce. The usual distinction between movement (leaves a trace) and deletion (evidently no trace) useful in syntax is actually, I assume (following Chomsky 1975), the distinction

between the presence of a semantic variable and its absence. For a justification of an underlying quantifier removed from site *t* in sentences like (74), see Bresnan (1975).) Note also the lack of contrast between (75), for those who can say it at all, and (76) or (77).

(75) Who'd you sen[<sup>Y</sup>] t your resumé?

(76) Who'd you sen[<sup>Y</sup>] your resumé to t?

(77) Where'd you sen[<sup>Y</sup>] your resumé t?

In a study of DP across the site of verb gapping, Cooper, Egido, and Paccia (1977) conclude from contrasts like

(78)(a) The bus driver will take your brother and then  
guide your sister.

(b) The bus driver will take your brother and the  
guide your sister.

(79)(a) The porter took your bags and weighed your  
luggage.

(b) The porter took your bags and Wade your luggage.

(their (9) and (6)) that an extraction site tends to block the rule. Note the natural pause in the (b) examples. The easily conceived idea that some more abstract state of affairs--like the presence of a trace--is responsible both for the pause and for blockage of DP is quite hard to seriously maintain. First, an extraction site is not always accompanied by a natural pause: (71)-(77). Second, DP is not inhibited precisely here. Third, DP is inhibited by all sorts of pauses, pauses which surely are not due to trace.

(80)(a) They've weighed...your luggage.

(b) It's all weighed. Your luggage is over there.

(c) Wade, your luggage is over there.

(d) After it was weighed, your luggage blew up.

Hesitations, ends of sentences, vocatives, and preposed adverbials naturally require pauses, and also make DP difficult. Since it is so clearly true that pause blocks DP, due to the failure of (10), it seems counterproductive to say that extraction sites also block DP (or even y-deletion), particularly when this theory is borne out only to the extent that pause is required as well.

Cooper et al. also point out that an actual silence of any length is not measurable in many instances of blocked DP. However, silence is not to be identified with pause. A pause is a thing of some abstraction, often surfacing as a lengthening of the pre-pausal syllable. Note:

(81) \*John telephone.

This is, of course, bad without a pause after the vocative.

(82) John, telephone.

But a silence of significant duration is not necessary. In rendering the comma of (82), it is possible to utter either (83)(a) or (83)(b).

(83)(a) John...telephone.

(b) Jo-ohn telephone.

The hiatus in phonation after Jo-ohn need not be large, or, it seems to me, even present at all. Note that lengthening in (78)(b) and (79)(b) seems virtually obligatory before the gap. According to Carmen Egido, of Cooper et al. (personal communi-



cation), their data in fact consistently show it.

v) DP is not even blocked between adjacent sentences in the same utterance.

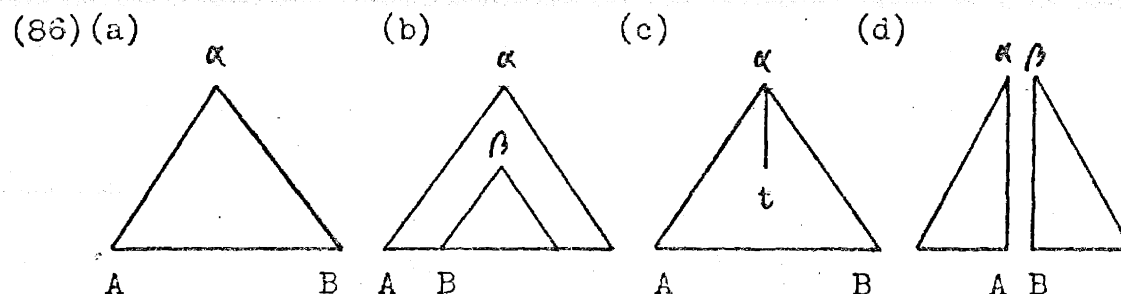
(84) Try i[<sup>✓</sup>c] (,) you'll live.

I assume that (84) is structurally no different from (85),

(85) Try it. You'll live.

in which the two sentences separated by the period are presumably not a syntactic constituent. The comma in (84) is merely an orthographic indication of their prosodic intimacy, which is exactly what DP requires.

In every single one of these properties DP distinguishes itself from the rules I've been worrying about in the rest of this work. There is no a priori reason why the absence of syntactic conditioning (ii) should entail (iii)-(v), or why the presence of syntactic conditioning should entail the contradictory propositions in case after case. I stress one more time that, according to my theory,



syntactic conditioning of a rule involving words A and B is of necessity dependent on their being separated by w-juncture, e.g. the one bounded by  $\alpha$ AB in (86)(a). But parenthesis (b), trace (c), and full stop (d) all have structural reflexes ruling out the possibility of any w-juncture inside a curve including A and B. On the other hand, pause (i), a prosodic

phenomenon, does not affect the normality of junctures, though it interferes with prosodic rules. DP turns out to be a prosodic rule, hence is predictably blocked by pause. To the extent that prosodic rules are not also affected by non-prosodic configurational conditioning (ii) (but recall the Italian example), (i) and (iii)-(v) will tend to exclude each other. One also imagines that there is no third case, i.e. that high-level phonology is sensitive either to prosodic structure or to non-prosodic, lexical structure. Hence a demonstration that a rule is not tied in to lexical structure should make us look for its prosodic conditioning, and vice versa. Note that even rules like the a/an rule, evidently without abstract limitations in the manner of Part III, are always conditioned by lexical structure in the sense that they are unproductive to an extreme degree. It ought to be impossible to lay such restrictions on a prosodic rule like DP.

I should mention that prosodic rules do not always have the superficial, phonetic air that DP has. An otherwise similar example is Finnish gemination, which appears to be conditioned by abstract segments.

(87) Mene [p:]ols!

go away

(88) Pane [p:]uita uuniin.

put wood in-the-stove

The words which cause this gemination belong to a list which one simply has to learn. Unlike the Irish case, a morphologi-

cal treatment is unattractive, since special syllable-final phonology also cooccurs with the ability to cause gemination. Nevertheless, the rule of gemination is defensibly conditioned by the analogue of Kahn's Rule V. It applies only in connected speech.

(89) \*Pane...[p:]uita uuniin.

Now note (90) and (91):

(90) Pane [k:]uule [p:]uita uuniin.

listen

'Put, hey, wood in the stove.'

(91) Mitä pitäisi panne t [p:]äällimmäiseksi?

what should-one put on-top

(Thanks to Paul Kiparsky.)

A word on the "psychological reality" of phonological rules. Looking naively at the vowel shift, for example, it is not the least bit clear that the set of examples constituting this phenomenon exhibit more than merely the sorts of fortuitous correspondences one would expect to find in any large body of inherited vocabulary. It is not obviously true that an authentic phonological rule synchronically accounts for them. The vowel shift has many exceptions, is not productive, is not supported by intuitions, etc. It is still possible to have an opinion about the nature of the vowel shift, ranging from the orthodox phonologism of SPE through various sorts of lexical correspondence theories (Vennemann 1972, Leben and Robinson 1977) to the extreme position that inane~inanity has about the same status as Cambridge~Cantabrigian. Now a

trans-word-level phonological rule is a phonological rule par excellence, because its productivity makes it difficult or impossible to think of as a lexical correspondence. DP, for example, is responsible for a completely productive alternation [XD]~[XP]. It is powerfully absurd to think that both of these are listed in the lexicon. Still needed is a means of specifying where [XP] actually occurs. If this means is a rule, the rule is DP, which makes /XP/'s lexical presence superfluous. But unlike the inane~inanity case, here it is not a serious alternative to list every [XP][yY]. Put another way, while one can think of inane~inanity as either (92)(a) or (92)(b), (93)(b) is simply ridiculous.

(92)(a) /ɪnæn/ : /ɪnæn/ity, plus VS

(b) /ɪneyn/ : /ɪnænɪtiy/

(93)(a) /bat/ : /bat/ you, plus DP

(b) /bat/ : /bɑ̃cuw/

The position on the vowel shift that [ɪnænɪtiy] is a monolithic word, without a constituent underlying inane, doesn't even have a counterpart on DP. We simply cannot get away without analyzing but in [bɑ̃cuw].

Now when we write the rule, which apparently we must, accounting for this palatalization of final dentals, we find that already it accounts for word-internal seize~seizure, etc. It's not so clear, just looking at it, that seize~seizure is a "real" alternation. But the fact that DP is general enough to account for it makes any lexical correspondence redundant. Hence DP constitutes a kind of evidence for word-internal

phonology.

An especially serious theory of lexical phonology is proposed in Leben and Robinson (1977). In this highly interesting and attractive system, surface forms are listed as is in the lexicon. /ɪnʌntɪy/, for example, is then related to /ɪneyn/ by interpretive phonological rules, essentially the inverses of the standard ones. Attempting to match /ɪneyn/ with the stem /ɪnʌn-/, we apply the inverse of the vowel shift, getting [ɪnʌyn]. (The complete derivation will also include the inverses of diphthongization and laxing.) This theory correctly predicts that the vowel shift will not be productive, because /ɪnʌntɪy/ has to already be in the lexicon for any rule to apply to it. Now imagine an analogous treatment of DP.

(94) /ɪneyn/ : /ɪnʌntɪy/, plus  $\overline{VS}$

(95) /bat/ : /bačuw/, plus  $\overline{DP}$

Bu[č] you, bu[č] your, bu[č] yet, bu[č] yesterday, coul[j] you, and every grammatical example of DP must be listed and submitted to rules first eliminating the word to the right of the palatalized consonant and then, via the inverse of DP, recovering but, could, etc., also listed. Absurd as this is, it also predicts that productive palatalization will not occur, which is not true. Unlike the vowel shift, DP quite naturally applies to borrowed, nonce-, and spontaneously made up words.

(96)(a) the fe[ž] you're wearing

(b) the skateboar[j] you're riding

(c) the gan[j] you ate

Therefore DP is in the active, rightside-up phonology. But then, by the same transitive reasoning above, so is DP when it applies word-internally.

## BIBLIOGRAPHY

- Akmajian, A., S. M. Steele, and T. Wasow (1977) "The Category AUX in Universal Grammar," ms., to appear in Linguistic Inquiry 9.
- Allen, W. S. (1962) Sandhi, Mouton & Co., The Hague.
- Anderson, S., and P. Kiparsky, eds. (1973) A Festschrift for Morris Halle, Holt, Rinehart & Winston, New York.
- Aoki, H. (1966) "Nez Perce Vowels and Proto-Sahaptian Vowel Harmony," Language 42, 759-67.
- Aronoff, M. (1976) Word Formation in Generative Grammar, Linguistic Inquiry Monograph #1, MIT Press, Cambridge, Mass.
- (1978) "Lexical Representations," ms., SUNY Stony Brook.
- Baker, L., and M. Brame (1972) "'Global Rules': A Rejoinder," Language 48, 51-75.
- Bolozky, S. (1977) "On the Status of Fast Speech in Natural Generative Phonology," in J. A. Kegl, D. Nash, and A. Zaenen, eds. (1977) Proceedings of the Seventh Annual Meeting of the North Eastern Linguistic Society, Cambridge, Mass.
- Brame, M. K. (1976) Conjectures and Refutations in Syntax and Semantics, Elsevier North-Holland, New York.
- Bresnan, J. (1971) "Contraction and the Transformational Cycle in English," ms., MIT.
- (1972) The Theory of Complementation in English Syntax, unpublished doctoral dissertation, MIT.
- (1975) "Comparative Deletion and Constraints on Transformations," Linguistic Analysis 1.1.
- (1976) "On the Form and Functioning of Transformations," Linguistic Inquiry 7, 3-40.
- (1977) "Toward a Realistic Theory of Transformational Grammar," in M. Halle, J. Bresnan, and G. Miller, eds., Linguistic Theory and Psychological Reality, MIT Press, Cambridge, Mass.

----- (to appear) Grammatical Interpretation.

Carrier, J. (1978) Morphology and Phonology in Tagalog: A Study of the Relationship of Rule Components in Generative Grammar, unpublished doctoral dissertation, MIT.

Cheng, C. C. (1973) A Synchronic Phonology of Mandarin Chinese, Mouton & Co., The Hague.

Chomsky, N. (1970) "Remarks on Nominalization," in R. Jacobs and P. Rosenbaum, eds. (1970) Readings in English Transformational Grammar, Ginn, Waltham, Mass.

----- (1973) "Conditions on Transformations," in Anderson and Kiparsky, eds. (1973).

----- (1975) "Conditions on Rules of Grammar," ms.; in Linguistic Analysis 2, 303-51.

----- (1977) "On WH-Movement," in A. Akmajian, P. Culicover, and T. Wasow, eds. (1977) Formal Syntax, Academic Press, New York.

-----, and M. Halle (1968) The Sound Pattern of English, Harper & Row, New York.

-----, and H. Lasnik (1978) "A Remark on Contraction," Linguistic Inquiry 9.

Clements, G. N. (1976) "Vowel Harmony in Nonlinear Generative Phonology," ms., Harvard.

----- (to appear) "Tone and Syntax in Ewe," in D. J. Napoli, ed., Tone, Stress, and Intonation, Georgetown University Press.

Cooper, W. E., C. Egido, and J. M. Paccia (1977) "Grammatical Conditioning of a Phonological Rule: Palatalization," ms., Research Laboratory of Electronics, MIT.

De Bhaldraithe, T. (1953) Gaeilge Chois Fhairrge: An Deilbhíocht, Institiúid Árd-Léinn Bhaile Atha Cliath, Bhaile Atha Cliath.

Dell, F. C. (1970) Les règles phonologiques tardives et la morphologie dérivationnelle du français, unpublished doctoral dissertation, MIT.

Dillon, M., and D. Ó Cróinín (1961) Teach Yourself Irish, English Universities Press Ltd.

Dotan, A. (1964) "Research in Biblical Accentuation, Backgrounds and Trends," Textus 4, 55-75.



----- (1972) "Masorah," in Encyclopedia Judaica, Macmillan & Co., New York.

Duncan, D. (to appear) Syllabic and Prosodic Structure, MIT.

Emonds, J. (1970) Root and Structure-Preserving Transformations, unpublished doctoral dissertation, MIT.

----- (1974) "Parenthetical Clauses," in C. Rohrer and N. Ruwet, eds. (1974) Actes du colloque Franco-allemande de grammaire transformationelle, Max Niemeyer Verlag, Tübingen.

----- (to appear) "The  $\bar{V}$ -V Complex in French," Linguistic Inquiry 9.

Erteschik, N. (1973) On the Nature of Island Constraints, unpublished doctoral dissertation, MIT.

Fidelholtz, J. L. (1975) "Word Frequency and Vowel Reduction in English," in R. E. Grossman, C. J. San, and T. J. Vance, eds. (1975) Papers from the Eleventh Regional Meeting of the Chicago Linguistic Society, Chicago.

Florinus, M. (1667) Doctrina de Accentuationa Divina.

Fouché, P. (1959) Traité de Prononciation Française, C. Klincksieck, Paris.

Hale, K. (1976) "Phonological Developments in Particular North Paman Languages," in P. Sutton, ed. (1976) Languages of Cape York, Australian Institute of Aboriginal Studies, Canberra.

Halle, M. (1977) "Tenseness, Vowel Shift, and the Phonology of the Back Vowels in Modern English," Linguistic Inquiry 8, 611-26.

Hankamer, J., and I. Sag (1976) "Deep and Surface Anaphora," Linguistic Inquiry 7, 391-428.

Harris, J. (1969) Spanish Phonology, MIT Press, Cambridge, Mass.

Harris, Z. (1947) Structural Linguistics, University of Chicago Press.

Hurford, J. (1971) Review of E. Bach and R. Harms, eds., Universals in Linguistic Theory, Journal of Linguistics 7, 132-45.

----- (1972) "The Diachronic Reordering of Phonological Rules," Journal of Linguistics 8, 293-5.

----- (1974) "The Base Form of English a/an: A Reply,"  
Lingua 33, 129-35.

Jackendoff, R. (1977) X̄ Syntax, Linguistic Inquiry Monograph #2, MIT Press, Cambridge, Mass.

Kahn, D. (1976) Syllable-Based Generalizations in English Phonology, unpublished doctoral dissertation, MIT.

Kaisse, E. (1977) Hiatus in Modern Greek, unpublished doctoral dissertation, Harvard.

Kayne, R. S. (1975) French Syntax, MIT Press, Cambridge, Mass.

Kenstowicz, M., and C. Kisseberth (1977) Topics in Phonological Theory, Academic Press, New York.

King, H. V. (1970) "On Blocking the Rules for Contraction in English," Linguistic Inquiry 1, 134-36.

Kiparsky, P. (1973) "'Elsewhere' in Phonology," in Anderson and Kiparsky, eds. (1973).

----- (1973a) "Abstractness, Opacity, and Global Rules," in proceedings of the Indiana University Conference on Rule Ordering, May 1973; available from the IU Linguistics Club.

----- (1974) "Phonological Representations," in O. Fujimura, ed., Three Dimensions in Linguistic Theory, Tokyo. }E

Klausenburger, J. (1974) "Rule Inversion, Opacity, and Conspiracies: French Liaison and Elision," Lingua 34, 167-79.

----- (1978) "French Linking Phenomena," Language 54, 21-40.

Labov, W. (1972) Sociolinguistic Patterns, University of Pennsylvania Press.

Lasnik, H., and R. Fiengo (1974) "Complement Object Deletion," Linguistic Inquiry 5, 535-72.

Leben, W. R., and O. W. Robinson (1977) "Upside-Down Phonology," Language 53, 1-20.

Lightfoot, D. (1976) "Trace Theory and Twice-Moved NP's," Linguistic Inquiry 7, 519-82.

Mascaró, J. (1976) Catalan Phonology and the Phonological Cycle, unpublished doctoral dissertation, MIT.

McCarthy, J. (to appear) "Expletive Infixes," in M. Aronoff and M.-L. Kean, eds., Juncture, MIT Press, Cambridge,

Mass.

McCawley, J. (1965) The Accentual System of Standard Japanese, unpublished doctoral dissertation, MIT.

Mhac an Fhailigh, É. (1968) The Irish of Erris Co., Mayo. Dublin Institute for Advanced Studies.

Milner, J. (1967) "French Truncation Rule," Quarterly Progress Report 86, Research Laboratory of Electronics, MIT.

Napoli, D. J., and M. Nespor (1977) "The Syntax of Raddoppia-mento Sintattico," ms., Georgetown University.

Orton, H., and E. Dieth (1962) Survey of English Dialects, University of Leeds.

Ostedal, M. (1962) "A Morphemic Evaluation of the Celtic Initial Mutations," Lochlann 2, 92-102.

Partee, B. (1973) "The Syntax and Semantics of Quotation," in Anderson and Kiparsky (1973).

Postal, P. (1974) On Raising, MIT Press, Cambridge, Mass.

-----, and G. Pullum (1977) "Traces and the Description of English Complementizer Contraction," Linguistic Inquiry 9.

Pyle, C. (1972) "On Eliminating BM's," in P. M. Parenteau, J. N. Levi, and G. L. Phares, eds. (1972) Proceedings from the Eighth Regional Meeting of the Chicago Linguistic Society, Chicago.

Ramanujan, A. K. (1967) "Two Kannada Styles," in Languages and Areas: Studies Presented to George V. Bobrinskoy, University of Chicago.

Reinhart, T. (1976) The Syntactic Domain of Anaphora, unpublished doctoral dissertation, MIT.

Ross, J. R. (1967) Constraints on Variables in Syntax, doctoral dissertation, MIT; reproduced by the Indiana Linguistics Club.

----- (1974) "Niching," ms., MIT.

Rosenberg, J. (1975) "French Liaison, Phrase Structure, and Semi-Cyclical Rules," ms., MIT.

----- (1976) "Semi-Cyclicity, Trace Theory, and Liaison: Continued," ms., MIT.

Rudes, B. (1976) "Lexical Representation and Variable Rules

- in Natural Generative Phonology," Glossa 10, 111-50.
- Saltarelli, M. (1970) A Phonology of Italian in a Generative Grammar, Mouton & Co., The Hague.
- Sapir, E., and M. Swadesh (1939) Nootka Texts 237.
- Schachter, P. (1976) "A Non-Transformational Account of Gerundive Nominals in English," Linguistic Inquiry 7, 205-41.
- Schane, S. (1968) French Phonology and Morphology, MIT Press, Cambridge, Mass.
- Selkirk, E. (1972) The Phrase Phonology of English and French, unpublished doctoral dissertation, MIT.
- (1974) "French Liaison and the  $\bar{X}$  Notation," Linguistic Inquiry 5, 573-90.
- (to appear) Phonology and Syntax: The Relation Between Sound and Structure, MIT Press, Cambridge, Mass.
- Sivertsen, E. (1960) Cockney Phonology, Oslo University Press.
- Spanier, A. (1927) Die Massoretischen Akzente, Akademie Verlag, Berlin.
- Spitzner, O. (1786) Institutiones ad Analyticam sacram textus Hebraici V. t. ex accentibus, Halle.
- Stanley, R. (1969) The Phonology of the Navaho Verb, unpublished doctoral dissertation, MIT.
- Trager, G. L., and B. Bloch (1941) "The Syllabic Phonemes of English," Language 17, 225-9.
- , and H. L. Smith (1951) An Outline of English Structure, Battenburg Press, Norman, Okla.
- Tranel, B. (1974) The Phonology of Nasal Vowels in Modern French, unpublished doctoral dissertation, UC San Diego.
- Trubetzkoy, N. (1939) Grundzüge der Phonologie, Travaux du Cercle Linguistique de Prague 7.
- Vennemann, T. (1972) "Rule Inversion," Lingua 29, 209-42.
- (1972a) "Phonological Concreteness in Natural Generative Grammar," in C.-J. Bailey and R. Shuy, eds., Toward Tomorrow's Linguistics, Georgetown University Press.

----- (1974) "Restructuring," Lingua 33, 137-56.

Vergnaud, J.-R. (1977) "A Formal Theory of Vowel Harmony," ms.

----- (to appear) "Formal Properties of Phonological Rules,"  
in R. E. Butts and J. Hintikka, eds., Proceedings of the  
Fifth International Congress of Logic, Methodology, and  
the Philosophy of Science.

Vogel, I. (1977) "Length Phenomena in Italian: Support for  
the Syllable," in W. Whistler et al., eds. (1977) Pro-  
ceedings of the Third Annual Meeting of the Berkeley  
Linguistic Society, Berkeley.

Wickes, W. (1881) Two Treatises on the Accentuation of the  
Old Testament, ed. A. Dotan, Ktav Publishing House,  
New York.